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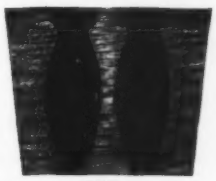

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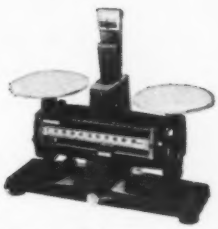

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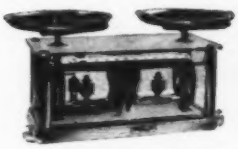

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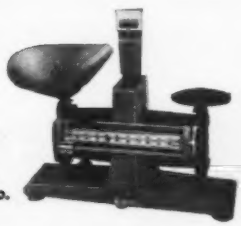

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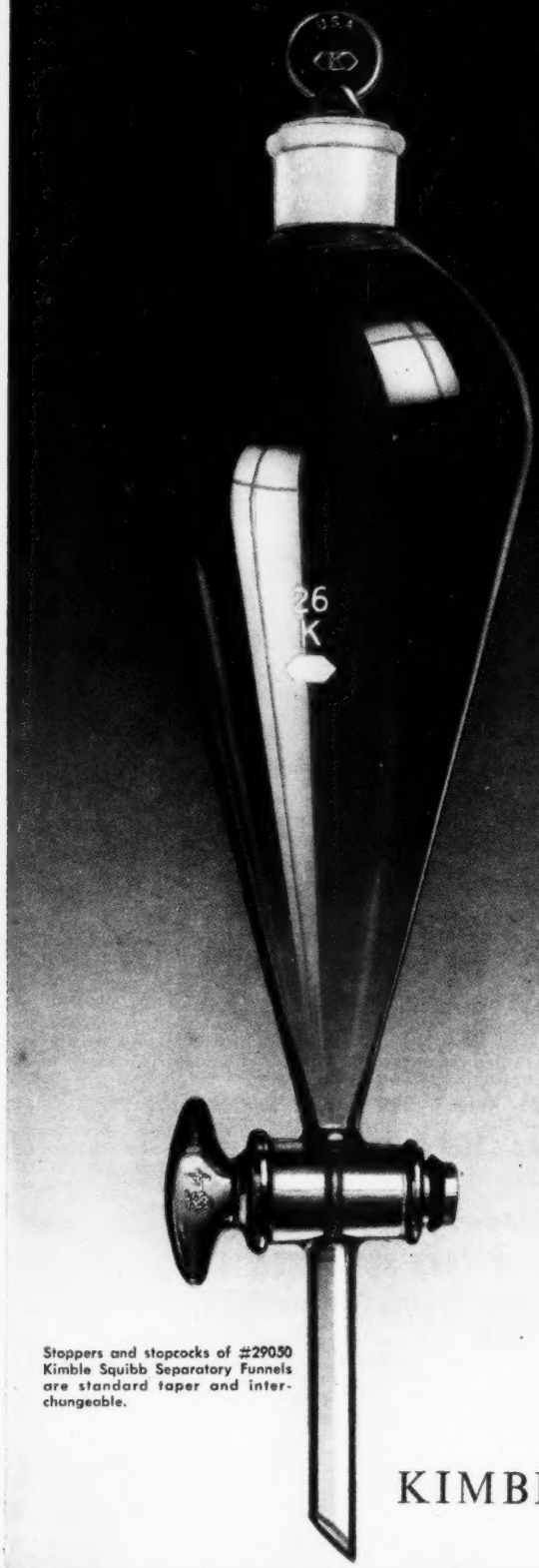

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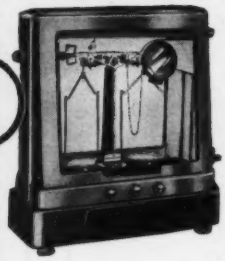

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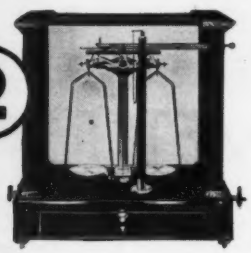

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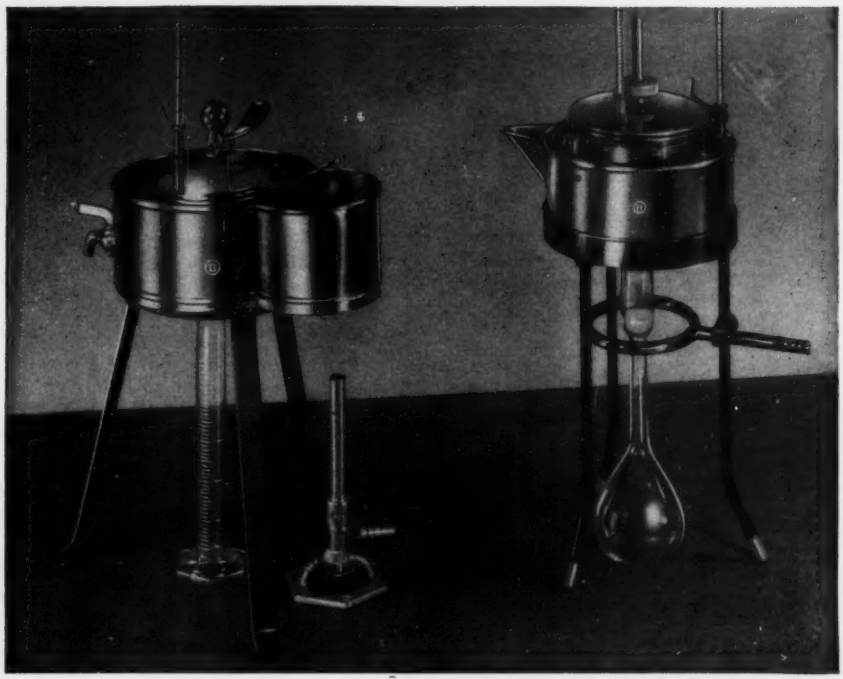

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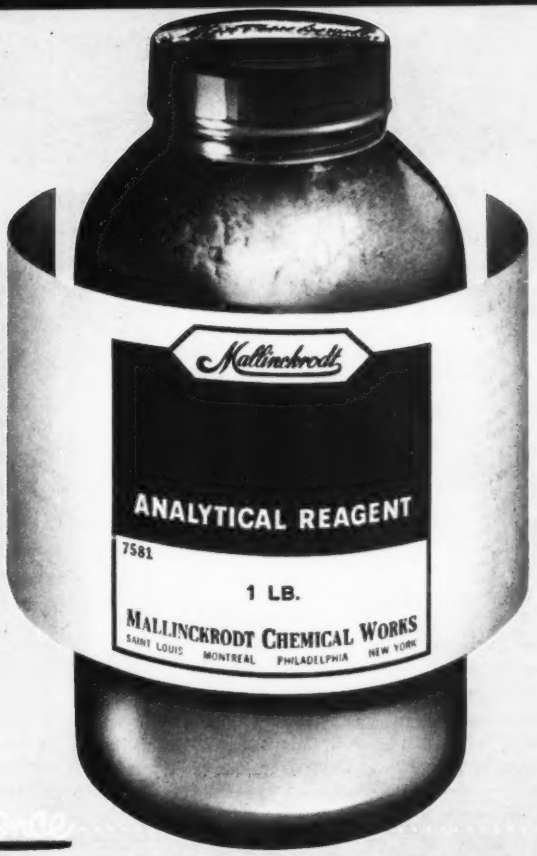

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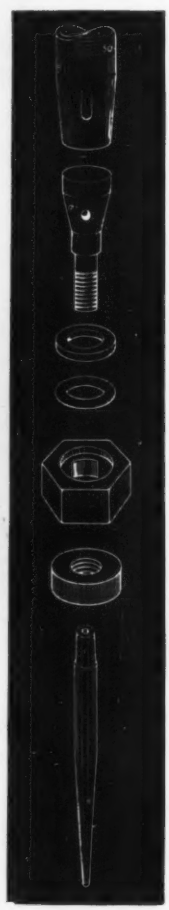

The valve assembly consists of a self-lubricating teflon plug seated in the tapered end of the buret tube. A channel is formed into the glass, enabling fluid passage through a side port of the plug when turned to the open position. The teflon plug is rotated by a large plastic turning knob, and is held in place by a locking nut threaded on the lower extension. When this locknut is tightened, the plug is pulled into contact with the tube taper, sealing tightly. A teflon washer adjacent to the glass, and a compression O-ring separate the plastic knob from the buret glass tubing. These last components facilitate easy turning and provide regulation of the sealing pressure.

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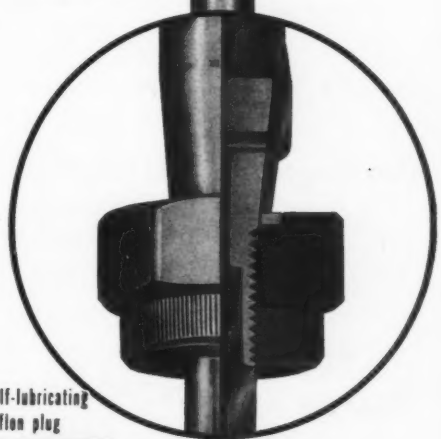

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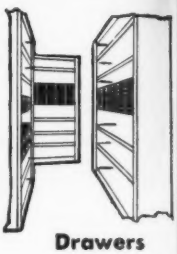

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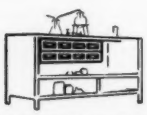

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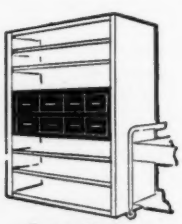

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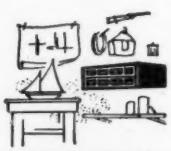

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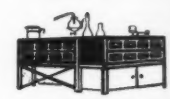

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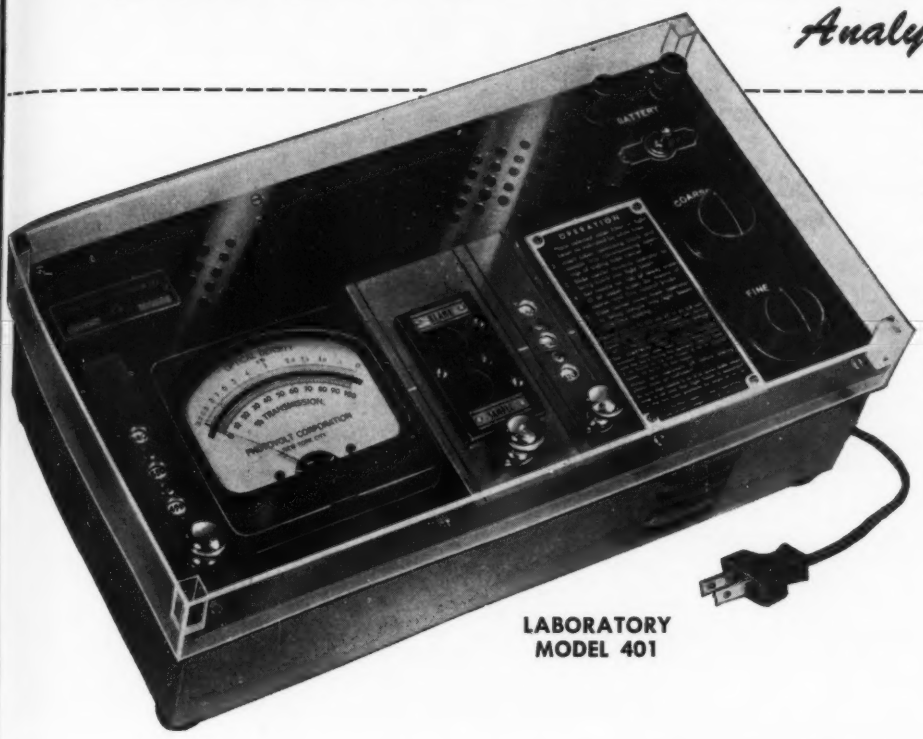

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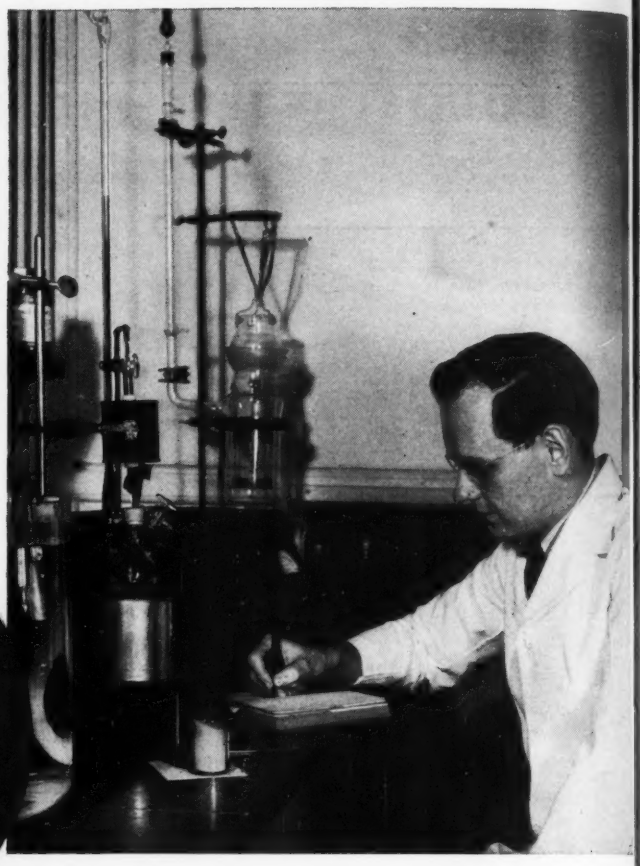

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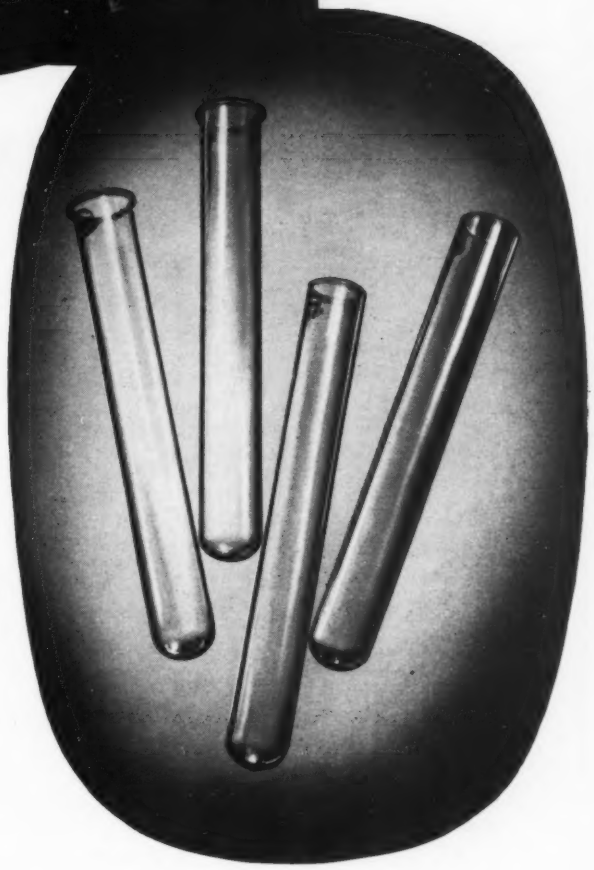

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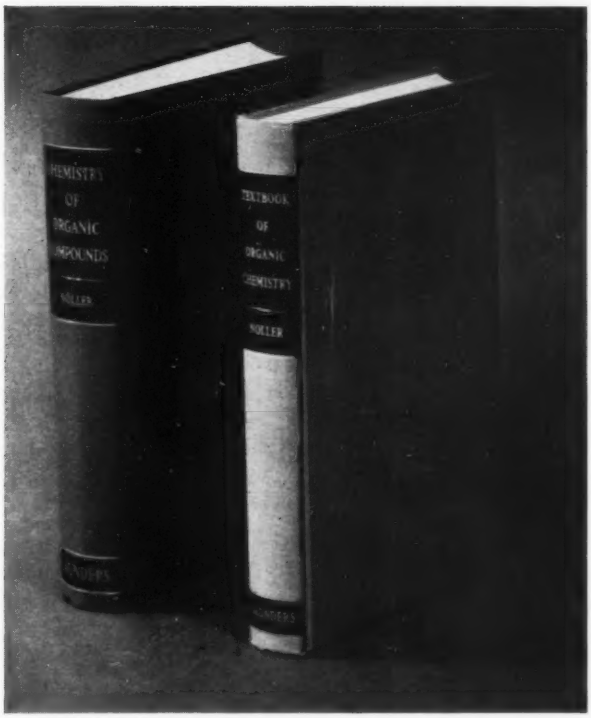

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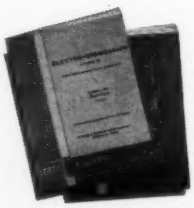

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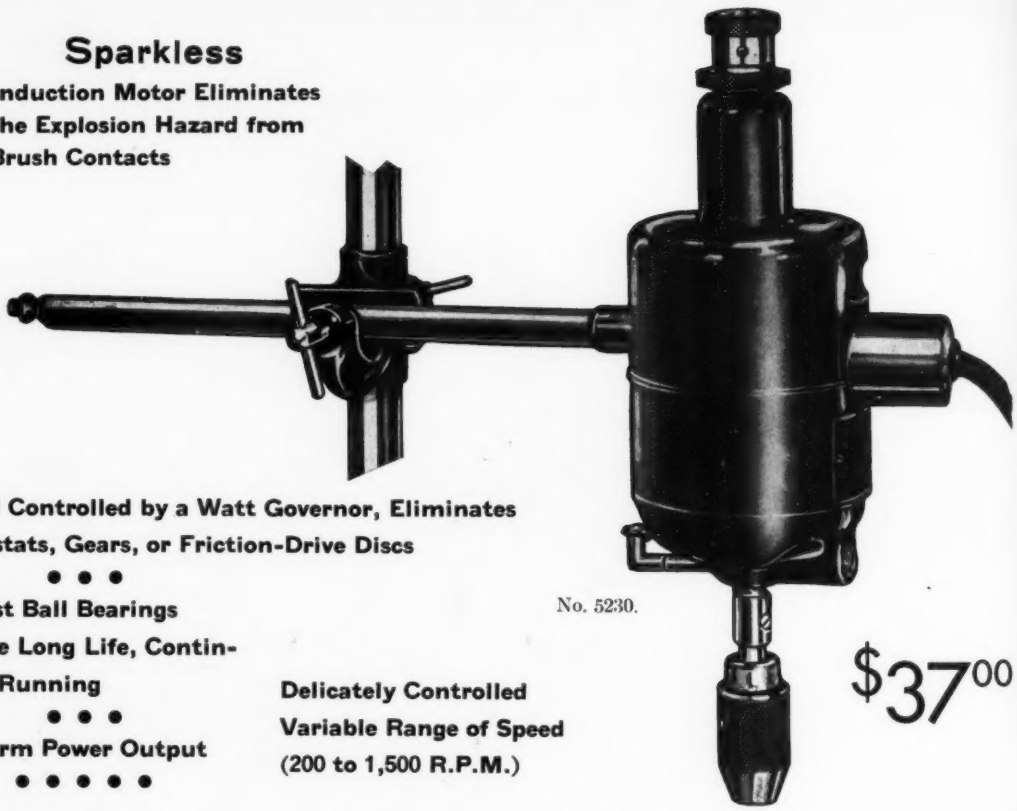

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Editor's Outlook

MUCH energy is being spent in devising a "general education" curriculum, at the higher as well as the secondary level. The curriculum makers have been heard to say that the justification for science in this pattern is the extent to which science instruction can contribute to the establishment and enjoyment of a beneficent society free from want and social restrictions. It is this modification of the old idea that "the end justifies the means" that I can't help but regard with suspicion.

Certainly, no one of us objects to the improvement of society and the betterment of the common lot of mankind. This poor and imperfect world needs a deal of tinkering. But when we set an improved society as our educational goal the next question is naturally, what *kind* of society, and then we involve ourselves in ideological conflicts. Pretty soon we have established a "party line" of our own, to which science, education, and all our other activities must adhere—or else.

There are values in science and science instruction which are inherent and intrinsic, independent of the application to which they are put or the motivation which they cause. Like the basic human rights, if we are not to be denied them we must be permitted to use them as we will, to our detriment or our advantage. Who wants to be *led* to heaven, blindly? This is the essence of "academic freedom," which is incompatible with a *planned* goal of any kind.

This is another instance in which we must choose our stand between the extremes of *laissez faire*, on the one hand, and a *planned society* on the other. Fundamentally, this compromise is perhaps the most important issue of our times.

*Symposium on the Contents of the Basic Courses in Chemistry**

THE CURRICULUM IN CHEMISTRY

PHILIP J. ELVING
The Pennsylvania State College, State College, Pennsylvania

In the 75 years which have passed since the founding of the American Chemical Society, there have been enormous changes in the scope and implementation of chemistry and chemical industry. Not the least of these changes has been the increased scope and variation of emphasis in the basic courses of the curriculum in chemistry. The changes in the chemistry curriculum have followed the development of chemistry, mirroring its uncertainties, hopes, disappointments, and triumphs. As new theories and new classes of materials have developed, the chemistry curriculum has reflected their coming and, occasionally, their passing. To illustrate, one need merely cite the appearance of courses in physical chemistry as a well-defined area in the latter part of the nineteenth century. The very titles of the courses in which physical chemistry was discussed and their subdivisions into more specified courses have clearly paralleled the development of this area. From the course in Theories of Chemistry came Theoretical Chemistry, then Physical Chemistry, and now a whole family of descendants: Chemical Kinetics, Thermodynamics, Statistical Mechanics, Quantum Mechanics, Colloid Chemistry, Phase Rule, Electrochemistry, Atomic and Molecular Structure, and so on, almost *ad infinitum*.

In an analogous fashion, the courses in analytical chemistry have reflected the emphasis placed on analytical chemistry 75 years ago when analysis was a predominant interest in all fields of chemistry in the efforts of chemists to define and understand the areas in which they were working and the materials with which they were concerned. As the interest shifted from the analytical aspect to the synthetic aspect, the courses in analytical chemistry decreased as the courses in organic chemistry increased. Then, as the newly developed field of physical chemistry increased, with its apparent promise of being able to explain all chemical phenomena from basic considerations, analytical chemistry further declined and many of the experiments which used to be done in analytical courses began to creep into other courses in chemistry. The nadir in analytical chemistry courses was probably reached some 20 years ago when little analytical chemistry was offered in most institutions beyond the first course in quantitative analysis. With the increasing need of the last decade for more exact knowledge of the constitution and composition of material, interest in analytical chemistry increased, and in recent years there has been an increasing multiplication in the courses offered in analytical chemistry.

The growth and change in undergraduate courses in chemistry can perhaps best be exemplified by the changes in the chemistry courses offered at one institution, The Pennsylvania State College, at the two extremes of the 75-year span of the American Chemical Society.

In 1876 the scientific curriculum at The Pennsylvania State College included one year of general and inorganic chemistry, one year of qualitative analysis, and one-third year of blowpipe analysis; an additional two-thirds of a year could be spent on qualitative analysis as an elective. These courses comprised the total offering in chemistry. In 1951 the Chemistry Department of The Pennsylvania State College offers at the undergraduate and graduate levels a total of 94 courses, which are augmented by the many chemistry courses given by the Department of Biochemistry and the School of Mineral Industries.

The growth in both the number and the variety of courses is evident and need not be underlined. It would be pertinent, though, to summarize the essential curriculum in chemistry specified by the American Chemical Society as a basis for approval for the offering of a major in chemistry.

The Committee on Professional Training of the A. C. S. has adopted the following standards for use as criteria in evaluating undergraduate training in chemistry. The minimum chemistry course requirements for the bachelor's degree must include four basic one-year courses in general chemistry, analytical chemistry, physical chemistry, and organic chemistry, plus at least one advanced course. The latter must necessitate as prerequisite three one-year courses in chemistry and may be in one or more of the following areas: inorganic, analytical, physical, or organic chemistry, or biochemistry. Qualitative analysis, that stormy petrel of the chemistry curriculum, may be given as a separate course or may be included in either of the courses in general chemistry or quantitative analysis.

* Presented at the 120th Meeting of the American Chemical Society, New York, September, 1951.

The purpose of the present symposium goes back to the series of symposia which the Division of Chemical Education has sponsored or co-sponsored in recent years. These symposia have dealt with the contents, function, and development of each of the basic courses given in the curriculum in chemistry. The purpose of the present symposium, then, is essentially that of summarizing and expanding the discussions and the ideas which were developed at these past symposia.

Each of the topics to be presented has been prepared with a three-fold aim. These aims may be itemized as follows:

The principal emphasis in each topic is on the delineation of the contemporary function of the basic course described, the place of this basic course in relation to the curriculum in chemistry, and the desirable manner in which the course might be improved.

Second, an attempt is made to furnish a definitive picture of the contemporary status of each of the basic courses and to indicate the pressures tending to modify it and the directions in which these courses will probably change.

Finally, the discussion aims to synthesize the viewpoints of various types of collegiate institutions, such as the large state university, the smaller endowed school, and the small liberal arts college. An attempt is made to avoid the type of presentation which has been briefly summarized as, "This is the way we do it at our school."

Obviously, our restriction in time places a burden on the authors in allowing them only to indicate the general trend in their discussion and to omit all but the most essential illustrative material. The symposium is limited to the basic undergraduate courses in the principal areas of chemistry. General chemistry is discussed from two viewpoints, since it was felt that the motivation for the terminal course in chemistry is apt to be quite different from that of the course in general chemistry which is taken as prerequisite to other courses in chemistry. Part of this difference in motivation is reflected in the increase in the last few years in the courses in inorganic chemistry which are being introduced into the latter part of the undergraduate curriculum in chemistry. This introduction is a most significant one and indicates a vital change in the chemistry curriculum.

THE COURSE IN GENERAL CHEMISTRY

MILTON TAMRES and JOHN C. BAILAR, Jr.
University of Illinois, Urbana, Illinois

STUDENTS beginning chemistry in most institutions are very different with respect to experience, ability, and professional interest. The first question which arises, then, in any analysis of the contents of a course in general chemistry is "Can a single course be designed to suit the needs of all students?" Since the terminal course is dealt with in another paper, we will consider the kind of general chemistry course which is followed by analytical chemistry, organic chemistry, and perhaps physical chemistry. Such a sequence is commonly given for premedical and predental students, as well as for those who are majoring in chemistry. To design a course solely for each of these groups, or, say, for home economics students or engineering students, etc., would not be practical at institutions with small enrollments, and there is some doubt that a wide variety of courses is wise under any condition. Our problem, then, is to teach a course which treats all students alike while recognizing their differences.

In approaching this problem we must remember that one aim of this kind of course is to prepare the student for later studies in chemistry, that is, the contents of the general course must be so integrated as to give the students a solid foundation upon which to build.

The atomic theory, including atomic structure, atomic and ionic sizes, the periodic table, and the nature of chemical binding, as well as the kinetic theory and the theory of equilibrium, are fundamental to a further understanding of chemistry and should be taught as early in the course as possible. Unfortunately, it is difficult to find suitable laboratory experiments for this part of the course. The students may devote some time to drawing atomic and ionic structures to scale, or working with models, but the teacher will probably find it necessary to introduce some descriptive matter into this early part of the course in order to be able to assign suitable laboratory work. Ideally, of course, classroom and laboratory hours would be flexible enough to allow perfect integration, but in

most cases college time-tables and teaching assignments do not allow this.

The stress on principles is made necessary because the facts of chemistry—properties and uses of chemical substances—have accumulated so rapidly that no one can hope to remember them except as they are summarized by the underlying principles. Even if they were remembered they would have little meaning to the students and could not be used to predict other properties. In general, therefore, descriptive facts should be introduced only as they correlate with principles.

A few examples may be used to clarify this thought. The average student will not care, nor will he long remember, that aluminum is more dense than magnesuim, or even that it is less reactive. A brief consideration of the structure of the atoms will show him, however, that the higher nuclear charge in the aluminum atom draws the electrons closer than in magnesium, and holds them more firmly. From this he can easily predict the relative densities and reactivities of the two metals, and can draw conclusions as to their relative boiling points, hardness, and the basicities of their hydroxides. Having completed this exercise he can extend his predictions to sodium and to silicon. With this background he will understand why sodium hydroxide is caustic. He may even use his imagination enough to wonder why one could not use aluminum hydroxide as a medicinal antacid.

Expansion of this lesson in atomic structure and atomic sizes will teach the students why calcium is more reactive than magnesium. Many of the properties of Group IIA now fall into a logical pattern; the students gain some insight into the growing importance of beryllium as a light structural metal. How can they help but want to predict the chemical properties of radium, or to compare rubidium with potassium? The danger in this is that students may try to predict too much from oversimplified ideas. It requires careful teaching to dissuade them from this without killing their enthusiasm.

The oxidation states of the transition elements, and the trend toward lower oxidation states as one passes from manganese to copper, can also be correlated with the changing structures of the atoms, helping the students to understand why ferrous salts are readily oxidized while cobaltous salts undergo oxidation only under certain conditions, and nickel salts hardly ever.

Carbon tetrachloride is unreactive toward water while silicon tetrachloride reacts vigorously. These are often presented as isolated and unrelated facts, although they are readily understood if we consider the relative sizes of the carbon and silicon atoms. The same consideration explains why carbon dioxide is a gas consisting of discrete molecules while silicon dioxide is a high-melting crystalline solid containing molecules of infinite size.

In like manner many chemical facts can be understood and remembered if they are made dependent upon Le Chatelier's principle and the theory of equilibrium. The facts that sodium chloride is soluble in water but insoluble in hydrochloric acid, that iron carbide becomes increasingly stable at higher temperatures while most molecules become less stable, and that ammonia can be synthesized only at high pressure, become logical when they are used as examples of Le Chatelier's principle. Also, the teaching of acid-base reactions can best be approached through the principles of equilibrium. Thus, the lowering of the acidity of a solution of acetic acid by the addition of sodium acetate can be explained by Le Chatelier's principle. Since we deal with aqueous systems, the Brønsted-Lowry theory should suffice. But the theory of equilibruim will be involved no matter which theory of acids and bases the teacher chooses.

The teaching of these fundamentals requires a certain minimum of mathematical treatment. It is not necessary to teach atomic structure from a mathematical point of view, but the concepts of stoichiometry, equivalent weights, derivation of formulas, the calculation of molecular weights, and other topics will not be understood by the student until he has actually solved mathematical problems involving them. The rigor with which these concepts are treated and the difficulty of the problems assigned will vary with the abilities and interests of the students and the amount of time which is to be devoted to the course. Whether such mathematical concepts as solubility product and change of pH through common ion effect are introduced will depend in large measure on the content of later courses.

Qualitative analysis can be used to emphasize the principles of atomic structure, ionic sizes, the periodic table, and the principles of equilibrium. This is well illustrated by the separation of Groups II and III, the amphoteric nature of the tin subgroup, and the formation of complexes. The complete schematic treatment of qualitative analysis should not be presented, this being included in a separate course whenever possible, preferably as a part of a sophomore analytical course.

The order in which descriptive topics are introduced may vary greatly, and is perhaps immaterial so long as some logical pattern is followed. Most teachers will probably prefer to start with oxygen and the other gaseous nonmetals, but those who are interested and versed in structural chemistry may prefer to begin with a discussion of metals, salts, or other crystalline substances. We can expect an increasing amount of time in general chemistry to be spent on structural features as more teachers become familiar with the concepts which are involved.

The distribution of time among the various descriptive topics is also subject to wide variation, depending upon the interests of the teacher and students. No harm will be done if the teacher elects to minimize the discussion of the chemistry of chromium in order to expand upon that of phosphorus, or if he omits some of the metallurgical processes in order to gain

time for the chemistry of glass. A class which is made up predominantly of engineering students will wish to devote a considerable amount of time to the chemistry of engineering materials, while a class composed chiefly of home economics students will prefer to spend more time on materials used in the home. This is quite permissible, since most of the descriptive matter is illustrative of theory for either group.

There are, it is true, certain exceptions to the rule that descriptive chemistry should be taught only as it illustrates theory. Some should be taught simply because it has student appeal or numerous "life contacts." Thus, the causes and effects of hardness in water, the poisonous nature of arsenic, of copper, and of barium compounds, and the yellow color of gold are not readily subject to theoretical interpretations in the course in general chemistry. But even in such topics as these principles should be invoked as much as possible.

The teacher who does not mention nuclear chemistry, the use of radioactive isotopes in biochemistry, or other topics of popular interest is missing an opportunity to whet the interest of his class. If the students are screened for ability and are deeply interested in chemistry some rather advanced material may be presented. This could include topics of special interest to the teacher or of current interest to chemists; for example, the hydrides of boron, nonaqueous systems, crystal chemistry, stereochemistry of complexes, intermetallic compounds, reactions in the solid state, etc. If properly presented, these discussions will enable the students to share in the enthusiasm for chemistry shown by the teacher.

An all-too-frequently neglected objective of the introductory course is the acquisition of a chemical vocabulary by the student. Unfortunately, most students do not enjoy this part of the study and will slight it if allowed to do so. On the other hand, many teachers do not realize the magnitude of the task which confronts the students in this regard and are inclined to be unsympathetic with them.

The usual course in general chemistry introduces more than 300 scientific terms which are new to the students and which they must thoroughly master during the year. (This count of three hundred does not include derivatives of words which have already been included in the count. Thus "oxygen," "oxide," "oxidation," "oxyacid," "oxychloride," and "oxyhydrogen" are counted as one.) If the mastery of 300 new words does not seem to be a heavy burden, it may be pointed out that this is about the number of words which the students will learn to use in the first year of study of a foreign language.

The number of new words in the general chemistry course cannot well be reduced because the ideas of chemistry cannot be mastered without them. However, since the task of learning them is formidable and the understanding of the course is dependent upon a thorough knowledge of them, the teacher should take care to assist the students in understanding their exact meanings.

ACKNOWLEDGMENT

Several of the ideas contained in this paper were developed in the Workshop on General Chemistry held June 13–22, 1951, at Oklahoma A & M College, which one of us (M. T.) attended. This is especially true of the suggestion in regard to the vocabulary problem. We wish to acknowledge our indebtedness to the members of the workshop.

THE TERMINAL COURSE IN CHEMISTRY

HUBERT N. ALYEA
Princeton University, Princeton, New Jersey

THIS discussion of the terminal course will be, like Gaul, divided into three parts: *who* is to be taught? *what* is to be taught? *how* is it to be taught?

WHO

Is your class a miscellany of nonscientists doomed, according to your standards, to a life of untruth and unreality? Do not apologize for them: you have a great responsibility. Are they future bankers? business men? politicians? *They* are the men who will make decisions of great importance for the future of chemistry. They will appropriate monies for research; and your terminal course will help them decide wisely. Are your terminal students "home ecs," beautiful but dumb? Treat them to the thrill of discovering that they, too, can understand science. Are they

poets? artists? Are they sensitively aware of beauty in music and art? Teach them the beauty of truth. Are they philosophers? Reveal to them the joy of a logic based on the simple premises of Nature's laws. Remember too: this may not be a terminal course for some. I had the pleasure of noting that two of the 12 honors seniors in Princeton last June came from the terminal course, for they had passed on to our more technical courses greatly matured in scientific attitudes.

WHAT

Here the speakers at the Chicago, September, 1950, meeting often disagreed. All use some case histories; in fact several universities had already been using this medium years before President Conant brilliantly described its potentialities in "On Understanding Science." Dr. Nash of Harvard, at the Chicago meeting, presented an eloquent plea for the historical approach,[1] using eighteenth century case histories uncluttered(?) by the intricacies of modern science. But eighteenth century case histories are not, by themselves, the whole answer; the student should share in the excitement of current discoveries as well. How can we expect to arouse his curiosity by a dissertation on dephlogisticated marine acid air (and no more) when he passes out of the classroom into a world full of vitamins, sex hormones, and atom bombs? In the Princeton course, for instance, we spend an entire week on the changing concept of hydrogen: from the four elements of the ancients, through phogiston of the Middle Ages, on to Cavendish discovering the compound nature of water, and especially tracing the rapidly changing twentieth century concepts of it—its atomic structure, atomic hydrogen and its reactions, application of quantum mechanics in predicting ortho-para hydrogen molecules, and the hydrogen isotopes, down to the radioactive hydrogen of today.

Dean Bent presented, at the Chicago meeting, a very sane appraisal of the needs of the terminal course: consideration of the future lives of the students, familiarizing them with descriptive material and nature's laws; caution lest the use of too much mathematics spoil their intellectual fun.

There are three kinds of subjects for the terminal course which I shall call normal, abnormal, and research topics. Normal topics are taught in every chemistry class: the orderliness of the periodic table, ionic size as related to atomic structure and its influence on chemical and physical properties, principles of equilibrium applied to some important industrial processes, and so on. These you know.

Abnormal topics are those peculiar to each teacher and his own interests. I have always been interested in kinetics: I can therefore teach my students more enthusiastically in that field than others. First and second order reaction rates—what strange topics for beginning students. No! for they sense my own enthusiasm and are thereby infected intellectually. A week on the three laws of thermodynamics. A week on the gas laws, not describing them in elementary word-pictures, but deriving $PV = nRT$ from the assumption that $kT = {}^1/_2mv$, and from that predicting the laws of Boyle and of Charles, and also the less familiar law which they themselves can "discover" as $P = kT$. Or you may be more interested in bond strengths, free energy, electromotive forces, dielectric constants, crystal structure, organic synthesis. No matter; you are scientifically excited and curious, and that is the law of infection.

The word "abnormal topics" was used advisedly, for we might also include "special, research topics" in the course. These are on a still higher plane of specialization. Professor Pauling brought this out in the discussion at the Chicago meeting: from time to time he will tell the students to fold up their notebooks, for they will not be quizzed; and then he launches into a dissertation on proteins, amino acids, and other highly specialized topics. I have done the same for my classes in the field of chain reactions and inhibition. If you will do the same in your own special field of research, the student will catch your spark of scientific enthusiasm.

This spirit of research must be injected into any terminal course which is to be alive. In addition you must help the nonscientist to transpose this spirit into his everyday life. Is it zeal to work? Listen to Robert Boyle:

> In my laboratory I find that water of Lethe which causes me to forget everything but the joy of making experiments.

Zeal inspires *hard work*. Mendeleev writes in the preface of his textbook:

> This investigation is dedicated to the memory of a mother by her youngest offspring. Conducting a factory she could educate him only by her own work. She instructed by example, corrected with love, and in order to devote him to science she left Siberia with him, spending thus her last resources and strength. When dying she said: Refrain from illusions, insist on work and not on words. Patiently search divine and scientific truth.

Oh yes, we must also have our dreamers. We all recall August Kekulé—the architect turned chemist—who continued to dream about molecular architecture:

> I was sitting writing at my textbook; but the work did not progress, my thots were elsewhere. I turned my chair to the fire and dozed . . . . the atoms were gamboling before my eyes . . . . My mental eye . . . . rendered more acute by repeated visions of this kind could now distinguish . . . .all twining and twisting in snake-like fashion. But look! What was that? One of the snakes had seized hold of its own tail, and the form whirled mockingly before my eyes. As if by a flash of lightening I awoke; and this time I also spent the rest of the night in working out the consequences of the hypothesis . . . . Let us learn to dream, gentlemen, then perhaps we shall find the truth . . . . but let us beware of publishing our dreams until they have been put to the proof by the waking understanding.

[1] Nash, L. K., J. CHEM. EDUC., **28**, 146 (1951).

Expect some *failures*—in science and research even as in life.

> The world [writes Michael Faraday] little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examinations.

The story of Ehrlich; of failure—utter and ignominious—before his great discovery of 606. Of Fletcher and his spoiled culture spotted with *Penicillium notatum* in which "he was sufficiently interested" to save the lives of millions of human beings. And then at last *success!* As Scheele writes:

> It is truth alone we desire to know; and what joy there is in discovering it.

Chemistry and Living; yes they can go hand in hand, and it is the task of the teacher to span these two, for the student. To fire him with zeal for something in life. To work and work through failure toward success. To push on after college years until we become authorities in something; for that surely brings happiness to man. This is a creed to bring to your students. A page from the chapter of science, in the Book of Life.

It is important, too, continually to remind these nonscientists of the inaccuracies of science and of the suspended judgment of science which says

> Say not it is the truth but so it seems to me to be as I now see the thing I think I see.

In general chemistry courses a proper balance must be struck between (1) arousing the student's curiosity in things scientific, (2) learning facts, (3) exercising his critical judgment. For the terminal course greater emphasis is to be placed on the first and the last, for facts are subservient. On the other hand, for the budding scientist, whose curiosity is already in evidence, the last two are more important, especially the learning of facts and the principles which substantiate those facts. For the student will have many opportunities to exercise critical judgment in more advanced science courses.

HOW

Lecture demonstrations? Of course. They are indispensable for arousing curiosity. But be sure to present these lecture experiments slowly, with deliberation, and with great pains that the demonstration and more particularly the principle is understood. It is not just a scientific show.

Laboratory? Naturally. Be you doers of the word and not hearers only. Not cookbook assignments but simple experiments which the student devises himself, no matter how crude and amateurish; observations which he makes and records himself, no matter how inaccurate and unreliable; conclusions which he formulates himself, no matter how fallacious. This is his own brain-child aborning; it is the growing-pains of his intellect, his real education. Is there much residue from our course left in the student ten years hence? That is the test of any course. For a whole month and a half we have our students carry out the iodine clock experiment: to search the literature in the library and make a complete report on their reading therefrom; to devise experimental details of how they will measure the effect of concentration, pH, temperature, catalysts, and inhibitors on the reaction; to carry out their measurements and report them in a formal paper suitable for submitting to the editor; and to list several research propositions. These young men, these nonscientists, submit astonishingly clever propositions. Out of them during the past two years have come the discovery of a clock reaction which first turns orange, then black; of a way to photosensitize the iodine clock to visible light; of the effects of heterogeneous catalysts on the reaction. Remember, nonscientists! Yet they will remember the original research they carried out in our chemistry laboratory long after they have forgotten those quotations from Byron, Shelley, or Keats.

Terminal course? May I quote now from a letter which I received this summer from a student in my terminal course. I hope it will be received in the spirit in which I write it: not for its praise, but for the sincere statement of what a young man may realize from a year's contact with science:

> If I were to say that I enjoyed the year of freshman chemistry immensely . . . . it is an understatement . . . . how much I appreciated . . . . how much happiness . . . . from being exposed to the material and thought . . . . I was not a good student; the B I received was gotten with considerable effort. But my difficulty was in mastering the mechanics of things like equilibrium constant, and not its role in human achievement and its value to humanity . . . . And how grateful I am for, "Say not this is the truth," etc., for that has formed what philosophy I have. People who think that they have a corner on truth amuse me! I have assumptions to go on; that which *seems* to be true. And you see how indebted I am, to you for it. I shall long remember Fleming's "I was sufficiently interested to pursue" and Pasteur's "Chance favors only the prepared mind," and on and on. That was the completely thrilling thing about chemistry to me. To think of the tireless probing of those men was at once refreshing to me.
>
> Let me quote Stephen Crane—one poem in particular reminds me of all real scientists:
>
> **Pursuing the Horizon**
>
> I saw a man pursuing the horizon—Round and round they sped.
> I was disturbed at this; I accosted the man.
> "It is futile," I said, "You can never . . ."
> "You lie" he cried, and ran on.
>
> More thanks than I can say for a thrilling year.

This letter was from an English major, a nonscientist. And so, you see, for at least one student in my terminal course in chemistry last year, the terminal course was only the beginning.

THE COURSE IN ANALYTICAL CHEMISTRY

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IT IS generally noted that analytical chemistry has undergone tremendous changes during the past ten years. The nature of these changes had made modifications inevitable in the teaching of chemical analysis. This paper is a review of the more important changes in course content and scope.

Of primary importance in the changing of status is the rebirth of respect and interest in analysis itself. Analytical work has become somewhat glamorized and has proved itself to be more than a routine control operation—it is now recognized as an important and challenging phase of scientific and industrial development.

The greatest single pedagogical change is the impact of "instrumental" methods, which are now so common in the universities. There seems little doubt that graduate courses covering instrumental techniques, microanalysis, coordination chemistry (complex ions, organic reagents), chromatography, etc., should be made available, and there are many who believe that undergraduate instruction in modern methods of analysis is also becoming an educational responsibility.

The second factor influencing present changes is the unsatisfactory status of qualitative analysis. For 100 years courses in "qualitative analysis" have been taught that have been based on the use of hydrogen sulfide separations. The analyses obtained through use of sulfide schemes have been so crude and the work so laborious that for many years the professional chemist has relied on spectroscopy, microscopy, and spot tests for qualitative determinations. Present qualitative analysis courses are impractical in many respects and have lost prestige. Many departments now offer only a "watered down" course or combine the qualitative analysis with freshman lectures, where they tend to become unrelated partners with little mutual aid. Some institutions are even dropping the qualitative work completely—a very serious error in the opinion of the author.

The preceding remarks will be considered in more detail.

TEACHING AIMS

Analytical chemistry has certain definite functions in the chemistry curriculum. The following aims are suggested as applicable in most departments in the United States and will at least suffice as a general guide for the present discussions.

Development and Practice of Common Laboratory Techniques. Analytical chemistry seems to be the field best suited for the teaching of the essentials of laboratory operations and disciplines. (There are cynics who feel it is the only place where technique is stressed.) A full year of analytical chemistry seems to be an absolute minimum for acquiring and establishing the required skills. This year of study can be made up of a semester of carefully taught qualitative analysis closely integrated with a semester of general quantitative analysis, two semesters of classical quantitative analysis, or a semester of classical methods followed by a course in instrumental methods of analysis. The latter combination is proving of real value in some departments in spite of the early predictions by some that only the traditional quantitative analysis courses could be used for establishing laboratory techniques. It is now clear that methods such as polarography and spectrophotometry require careful attention to the art, just the same as titrimetry, and in fact have some special value because the necessary practice is provided under conditions especially interesting to the students.

Of real interest at this point is the trend toward the teaching of semimicro methods in the general chemistry laboratory. Such methods seem to aid the analytical chemistry faculty through the early introduction of more defined manipulative requirements.

Development of Calculation Skills. The need for training in the methods of chemical calculations is too obvious to require lengthy exposition here. Computations are essential to the understanding of theoretical principles and are almost mandatory for the ultimate appraisal of data accumulated in the analytical laboratory.[1] The disciplines of chemical calculations are of far reaching value, and it is often overlooked that one of the best ways of adding breadth to studies of analytical chemistry is to present the essential steps for many procedures in the form of labeled data which are to be used in subsequent computations.

Revolutionary changes are not taking place in chemical calculations. It is noteworthy, however, that there is increasing interest in teaching titers in the United States, where normalities (milliequivalents) have long been in rather exclusive use. The use of titers has been the established practice among European chemists for many years and has also had wide industrial application in this country.

Sound Philosophies of Scientific Investigation. Such questions as why, what, and how much are part of the life of the scientist. His training must give him a conviction that he is a dealer in *truth*—one correct answer is better than 100 that are wrong.

No changes in the teaching of this aspect of analytical chemistry can be reported. Here, above all, the role of the professor becomes of prime importance: the inspiration of an understanding and helpful instructor

[1] WEST, P. W., J. CHEM. EDUC., **28**, 437 (1951).

can be the determining factor in establishing the ultimate worth of a fledgling scientist.

The Teaching of Chemistry. All too often we overlook the fact that it is chemistry we teach and not analytical, or physical, or organic chemistry. The professor of analytical chemistry is certainly responsible for making available to his students the pertinent reactions involved in assigned laboratory work. In addition, he should be provided with enough lecture time to develop those theoretical principles of general importance that can be presented effectively at the instructional level concerned.

There seems to be a definite trend toward more emphasis on lecture work. With careful integration of subject matter the increased lecture time can serve a most useful function in equalizing the study load within the scope of the basic curricula of chemical, premedical, and engineering studies. For example, some of the difficulties connected with the crowded physical chemistry courses can be obviated by more thorough coverage of some of the principles of chemistry in the analytical courses.

Miscellaneous Aims. One function of analytical chemistry courses might very well be to find superior students and attract them into chemistry. An established aim, but one losing in favor, is to provide experience in "practical" analytical problems. This sounds plausible until it is realized that such experience can be gained only at the expense of time from other studies. Under this heading may appear certain special aims that apply to special service courses.

PRESENT STATUS

Quantitative Analysis. The normal development of analytical chemistry was tremendously accelerated during World War II, with a sudden impact on teaching methods. Some colleges quickly adjusted their curricula and courses, while others did not. Analytical programs were reviewed mainly at the graduate level, so that fewer students and fewer new instruments would be involved. Furthermore, the graduate program is usually relatively flexible and permits more freedom than the tight schedules of undergraduate curricula.

A few courses in instrumental analysis have been added at the undergraduate-graduate level,[2] while a number of departments have solved the problem by introducing some instrumental work as part of existing courses.

It is remarkable to find that most of those who have taught instrumental analysis or newer techniques in noninstrumental methods of analysis, have concluded that the results obtained are very satisfactory. However, a word of caution at this point is needed: the modern methods of analysis do not supplant classical methods, they merely supplement them.

Qualitative Analysis. Qualitative analysis raises a number of controversial questions, one of the first being "What is it?" As it now stands, the course would more properly be labeled "Analytical Separations" or "Systematic Inorganic Chemistry." The methods taught in college are so inadequate as qualitative tools that scarcely a single industrial laboratory remains in which any of the qualitative analyses are run with classical procedures. To make the situation especially dangerous, it is seldom that the average student has the opportunity to elect the more practical courses dealing with microscopy, spectroscopy, and spot tests. As a result, there are a number of chemists now who feel that qualitative analyses are not important because they are so seldom run in the industrial laboratory. Overlooked is the fact that the missing stimulus is not the need but the skill.

The value of the qualitative courses as a teaching aid is universally recognized. The current need is for an approach that will retain the pedagogic value of the present course and will include, in addition, material of practical value in qualitative identifications and quantitative separations. Perhaps some of the new non-hydrogen-sulfide schemes will provide part of the answer.

CONCLUSIONS

The integration of instrumental methods of analysis with classical methods has presented a challenge to the teachers of analytical chemistry. Without question, there will continue to be an increase in the varieties of instruments introduced at both the undergraduate and graduate levels, although there can be no complete breakaway from the traditional courses. The swing to instrumental techniques affects the chemistry majors particularly, although there would be some advantage in introducing premedical students to a few of the basic instruments.

Enough time has elapsed to permit an evaluation of the worth of instrumental methods from the instructional viewpoint. Very few would doubt the value of such work at the graduate level, and there are now indicated a number of advantages associated with the introduction of such studies at the undergraduate level. Its technique can be practical and its theory can be significant.

Every effort should be made to retain qualitative analysis as a basic course in chemistry, perhaps with new aims and methods. Future courses should be made to correlate more closely with methods of quantitative analysis. Certainly more workable methods of actual qualitative identification should be taught, and hydrogen sulfide separations need not be the only scheme studied.

ACKNOWLEDGMENT

The author wishes to express his appreciation to Dean A. R. Choppin, who has pioneered in many of the changes in the analytical chemistry curriculum, and to his colleagues, Paul Delahay, Arthur L. LeRosen, Gunther Eichhorn, and Maurice M. Vick, who have have made valuable comment.

[2] CHOPPIN, A. R., A. L. LEROSEN, AND P. W. WEST, *Anal. Chem.*, 19, 640 (1947).

THE COURSE IN ORGANIC CHEMISTRY

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WE MIGHT start our discussion by asking the question: What is the function of the first course in organic chemistry? I am sure that there are as many different answers as there are teachers in the field. In my opinion, the object of the first course may be stated quite simply. It is to *introduce* the student to the science of organic chemistry by a discussion of both factual and theoretical material. The key word in this statement of the objectives of the course is *introduce*. With more and more facts being added rapidly to the already vast area of organic chemistry, we teachers are faced with the almost insoluble dilemma of deciding what material should be stressed in the introductory course. To get some idea as to the amount of time which is devoted to the elementary organic course I prepared a questionnaire which was sent to 50 colleges and universities. From the 42 replies it was found that from two to four hours a week are spent in the classroom. In some schools one-half to one hour of this time is spent in recitation. From three to seven hours a week are devoted to laboratory work.

As we all know, the number of classroom hours has not been increased appreciably during the past 15 years, yet the size of our textbooks has increased tremendously. In addition to the many new facts which have been added to our already vast storehouse of organic chemical knowledge, our textbooks now devote considerable space to a discussion of the mechanisms of organic reactions. The result is that our texts have become almost encyclopedic in nature. The teaching of organic chemistry has become somewhat of a contest, with the teacher and student on opposing teams. The teacher, confronted with a large text, is racing against time to cover as much of it as he can; the serious student, desirous of learning and getting the best grade he can, spends the lecture periods writing like fury. He copies equations, structural and electronic formulas, and proposed mechanisms for reactions so rapidly that he understands and follows very little of the material as it is presented in class. It is not at all uncommon to hear students make statements such as the following: "He writes with one hand and erases with the other," or, "You don't dare drop a pencil in his class, because you can miss two weeks of chemistry before you pick it up." Unfortunately, there is more than a slight element of truth in these complaints by our students.

The question now arises: Just what can be done about our dilemma? The answer to this question is of course intimately associated with the amount of subject matter to be covered in our lectures and the method by which the material is to be presented. The two most common methods of presenting the lecture part of the course are: (1) the classical method and (2) the mechanistic approach. In both of these methods the lectures involve a detailed treatment of aliphatic and aromatic compounds. The discussion of the aliphatics and aromatics is usually bridged by a treatment of optical and geometrical isomerism, amino acids, proteins, and carbohydrates. If time permits, and I really don't see how this is possible, the chemistry of dyes, polynuclear hydrocarbons, alicyclic and heterocyclic compounds is discussed briefly.

The classical and mechanistic approach differ in that the former is essentially descriptive. It details the various types and reactions of organic compounds and little or no stress is placed on a discussion of the course by which reactions take place. In the mechanistic approach, the ordinary descriptive material is supplemented by the use of electronic formulas, resonance theory, and the Brønsted and Lewis theories of acids and bases as aids in the understanding of the course of organic reactions and in the prediction, in a qualitative manner, of the physical and chemical behavior of organic molecules.

Having taught organic chemistry by both methods, there is little doubt in my mind that the approach involving electronic theory is the more desirable of the two. Perhaps the main objection to the classical approach is the result that students attempt, and sometimes even successfully, to memorize the textbook and the lecture notes. They fail to see any logic or principles in organic chemistry, and the result is that organic chemistry is all too often referred to as the great empirical branch of chemical science. The extensive use of electronic theory and resonance concepts in the elementary course has the great advantage that the student realizes that organic chemistry is not a science made up of thousands of unrelated facts. He finds that electronic theory is of great value in explaining the reactions of organic chemistry. Thus, for example, the chorination and bromination of benzene in the presence of an iron catalyst, the nitration and sulfonation of benzene and the Lewis acid-catalyzed acylation and alkylation of benzene—reactions which at first sight seem to be quite different—may be explained and understood on a common basis by application of electronic theory. The theory is also of great value in explaining the greater ease of substitution of phenol when compared with benzene. The student thus finds that the use of electronic theory replaces memory to some extent by the application of principles. However, since electronic theory is useful in explaining facts and predicting, for example, what may occur when compounds A and B are allowed to react under previously unreported condi-

tions, it is important to keep in mind that the study of chemical reactions and syntheses must not be relegated to a place of secondary importance in the first course.

The question still remains of solving the problem of giving a good first course in the few hours a week which we have at our disposal. The following suggestions may be worthy of consideration:

(1) The most obvious solution would be to increase the number of hours devoted to organic chemistry. However, such a thought is not a realistic one since not only the organic chemistry course but also the other elementary chemistry courses, as well as courses in the other physical sciences which our students are required to take, would benefit from more time.

(2) A second approach is a combination of the American and European plans of study. In this connection, the following excerpt from a letter written to me early in 1950 by Professor Brewster of the University of Kansas, the author of two of our most popular elementary textbooks, is of interest.

> The problem of covering all the material that one desires seems to be almost insoluble. The closest approach to the desired objective that I have ever been able to obtain has been made by a combination of the American system of teaching and the European plan of individual study; hence, I most commonly assign 8 to 10 pages of the text but limit the discussion in class to the major points on those pages. The minor topics are explained only if the students raise a question about them.
>
> According to my own experience in teaching most all kinds of classes in Organic Chemistry, it seems that I covered comparatively little ground when I tried to present most of the material by the lecture method. On the other hand, when I assigned a certain number of pages in the text and lectured briefly upon certain main topics, with the students getting the rest of the material from the textbook, the amount of material covered was much greater and the students' mastery of it was much better than when I took too much of the burden of teaching upon myself.

This suggestion merits a lot of consideration since it no doubt will enable the professor to cover much more material in the allotted lecture time. However, there may be some pitfalls in its use. I feel that it can be really successful only with superior students. To expect the average college sophomore or junior to read scientific material intelligently and critically is perhaps expecting too much. Such reading is further complicated by the students' misinterpretation of statements made by textbook authors. The ammonolysis of an acid chloride is an example of what I have in mind. Many authors show the reaction of one mol of an acid halide with one of ammonia to give a mol of an amide and a mol of HCl. The purpose of such an equation is to show the formal relationship between ammonolysis and the closely related reaction, hydrolysis. The authors do not mean to imply that ammonia and HCl coexist without reaction. Yet, unless we stress that the reaction gives ammonium chloride, the student is confused and learns something which is not true.

(3) Another possible solution to the problem of presenting the desired material in our limited classroom time has been suggested by Professor Norman Kharasch of the University of Southern California. In a recent paper[1] he reports that he has taught organic chemistry to undergraduates effectively by the use of lantern slides coupled with a set of guide notes.

So far, I have mentioned three solutions to our dilemma of trying to give a comprehensive course in the available lecture time. However, there is another point of view about the first course which should be considered very seriously. This is that the first course should not be comprehensive but should really be an introduction to organic chemistry. According to this point of view, we teachers should not attempt to discuss all the reactions of organic chemistry. We should select a limited body of factual and theoretical material and discuss it very thoroughly. If we make the scope of our introductory course more modest, it will also be possible to go into the mechanism of many of the reactions which we cover. In this way our students will get to understand thoroughly a core of elementary organic chemistry. Since these students are to become professional chemists, later in their studies they will get detailed information on many of the topics which are discussed very briefly or not at all in the first course. A textbook[2] embodying these ideas was recently written by Professors English and Cassidy of Yale University. In this text, there is also a combined treatment of aliphatic and aromatic compounds, since these authors feel that the differences between these classes of compounds are of a quantitative rather than of a qualitative nature.

Now a few words about the laboratory work in the elementary course. There are essentially two methods which have been used in laboratory instruction: (*a*) semimicro techniques and (*b*) macro techniques. The foremost proponent of the semimicro techniques is Professor Cheronis of Brooklyn College. He discussed his views[3] in a symposium at the September, 1950, meeting of the American Chemical Society. He feels that:

> The use of small quantities (*a*) reduces materially the time required for most operations . . .; (*b*) it instills in the students superior habits of care, cleanliness and manipulative skill; it reduces the seriousness of possible accidents; (*c*) it reduces the cost of laboratory instruction.

In the macro approach to teaching laboratory work emphasis is on the preparation of typical organic compounds on a macro scale. In many of these courses a compound X is converted to a compound Y by a series of chemical transformations. In each step of the synthesis the intermediates are isolated and the yields determined. A student performing preparations on a macro scale gets a heart warming feeling of accomplishment when, as a result of his labors, he can point with pride to a bottle containing a large sample of a pure liquid or a crystalline solid.

At the present time the macro method is the more

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[1] KHARASCH, N., J. CHEM. EDUC., **28**, 280 (1951).

[2] ENGLISH, J., AND H. G. CASSIDY, "Principles of Organic Chemistry," McGraw-Hill Book Co., Inc., New York, **1949.**

[3] CHERONIS, N. D., Abstracts of 118th A. C. S. Meeting, September, 1950, p. 14E.

widely used laboratory method of instruction. An argument which has been advanced in favor of the macro method is that we are training men for industry and there they will rarely perform a semimicro synthesis. However, in all fairness to the supporters of the semimicro approach, it should be stated that[4] "experience with students who have had introductory training in the semimicro techniques has shown ease of adapting macro methods when such necessity arose."

If this discussion of some of the problems which confront us teachers of organic chemistry has been of help to just one of my readers, then the purpose of having prepared this paper has been accomplished.

[4] CHERONIS, N. D., *op. cit.*

THE PHYSICAL CHEMISTRY COURSE

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OBJECTIVES

The function of the basic course in physical chemistry is to introduce the student to theoretical chemistry. More specifically, this introduction should accomplish several things. First, it should provide the information necessary for the correlation and the extension on a theoretic level of a student's previous variety of chemical experience. In the second place, it should make clear the ultimate problems of physical chemistry: to calculate positions of chemical equilibrium and the rates of their attainment. Finally, the course should develop a recognition and a reasonable familiarity with the methods of attack which physical chemistry provides for the investigation of all chemical phenomena. It can easily be argued that the treatment accorded topics in the physical chemistry course represents a fusion or interlocking of these functions at nearly every instance. This course is more than a mere high-powered review of previously acquired information, mainly because as the student becomes sufficiently familiar with the line of attack which theoretical chemistry follows, he begins to realize what additional information is required to establish a valid explanation of phenomena. The correlating function is better accomplished subtly throughout the course than by obviously organized lectures on the applications of physical chemistry to analytical or organic chemistry *per se*. The important point of view is for the professor to hope that few items in a student's previous chemical experience will remain as unique or isolated phenomena.

The position of this discussion in this symposium implies that physical is somewhat of a climax to the undergraduate curriculum in chemistry. With respect to prerequisites (physics, calculus, and usually two years of chemistry) this may be true. It often is the course which rounds out the student's introduction to the varieties of chemistry, since the only undergraduate courses usually taken after physical are some of the analytical, advanced inorganic, and whatever advanced organic is available. In some colleges where physical is the "senior course" it is considered by faculty and students alike to be the climax to the collegiate training. The extreme contrast to this is found in the university "tool" course. It must be taken to qualify for graduate courses open to undergraduates. In spite of this apparent divergence in aim, the point of view of physical chemistry professors toward what they want to accomplish is remarkably uniform across the country. The trend seems to be for all types of institutions to put physical in the third year where it can compromise as a mature coordination of other courses and also as a necessary prelude to research or more advanced specialized courses.

It will be recalled that there are two essentially distinct types of scientific thinking which are exercised in any complete theoretical analysis of natural phenomena. The first of these is the mechanistic elaboration of the details of the process being investigated. A model is invented and the logical consequences of its assumptions reveal a picture of reality which is more or less successful depending on the astuteness with which the model is conceived. Virtually all of the student's chemical experience before his physical chemistry course is based on this type of approach. It is no real jolt to him when in the early lectures the professor goes on from the simple kinetic equation to show how the distribution of molecular velocities depends on the velocity even though some of the student's less familiar mathematical resources may have been tapped to grasp the development. As long as he can think mechanistically the student has

little difficulty in re-examining with a new rigor, and even pushing on into new areas of exploration. Van der Waal's forces, deviations from Raoult's law, etc., may appear more complicated yet they are essentially familiar arguments.

The big intellectual hurdle for the student beginning physical chemistry is the demands of a method of reasoning which precludes any perfection or imperfection in his model. Thermodynamics epitomizes this empirical type of scientific thinking. This is utterly new to the student. He is required to manipulate the symbols for such abstract entities as heat and temperature by the reasonable but often remote operations of the calculus to yield what appear to be compound abstractions such as entropy, free energy, and heat content. Not only does the student feel far removed from his familiar test tubes and end points, but it is difficult for him to be other than distrustful at first of laws which seem to be generated out of integral signs and tables of data. The greatest challenge to the professor of physical chemistry is to establish a solid relationship in the student's mind between the abstractions themselves and the phenomena upon which they are based. It is here that the real scientific maturity of the student chemist develops. "The dawn breaks" on what calculus is all about, not as a result of formal lectures on its application to chemistry, nor even by repeated integration of the Clausius equation. It occurs because the student uses calculus not merely to get an answer by a convenient method but to develop a necessary set of concepts. The partial differentials for heat capacities and the graphical methods of integration to yield entropies are outstanding examples.

SUBJECT MATTER CONTENT

The scope of thermodynamics has been so widened in recent years that at least the significance of its application to more and more chemical problems has found its way into the basic physical chemistry course. Thermodynamics may well be considered as the mortar which holds together the informational bricks of the course structure. The free-energy concept is the most notable example. Le Chatelier's principle and the arbitrary equilibria of the student's freshman course, the ether extractions of his organic laboratory, and the electrolytic precipitation of copper ahead of zinc in the analytical laboratory all reappear in the physical course. They begin to fit together, each as a special case of a broad generalized principle. Correlation is possible only by extending one's knowledge to include concepts which depend quantitatively on the energetics of equilibria.

Just as a consistent point of view toward the objectives of the course seems to be shared by professors in various types of institutions, so too there appear to be no gross disagreements about the relative amounts of time to be devoted to the different portions of the subject matter content. The sequence of topics will vary depending upon choice of text, extent of coincidence with laboratory work, etc. The trend to emphasize thermodynamic approaches generally places the development of the fundamental laws early in the first term. This also may result in the reshuffling of the traditional order of discussing certain properties of the states of matter. For example, the vapor pressure of liquids and the phenomenon of azeotropy may find a place in the discussions of systems in equilibrium rather in their respective traditional chapters on the liquid state and solutions. As has been pointed out above, physical chemistry complements other courses, so that the extent to which any particular subject will be treated will invariably reflect the adequacy of its introduction and theoretical treatment in other courses at any given institution. Trends are apparent here too. Much of the ionic equilibrium information which used to be part of the physical course is now treated in the classes in analytical. Some of the essential information of electrochemistry may have the same fate. A consequence of this is to have the physical course take up where the others leave off, especially by introducing the activity concept. This again pulls thermodynamics into the picture and emphasizes the generality of its equations without arbitrary and individual corrections to bring them in line with observed behavior. For example, it may startle but probably will reassure the student to calculate the salt errors he neglected back in his K_{sp} calculations of qualitative analysis.

A composite estimate of time distribution for the course can be stated in the following round numbers. Sequence is not implied.

States of matter and solutions	25–30%
The laws of thermodynamics, free energy, and equilibrium	30–35%
Kinetics and theory of reaction rate	10–15%
Electrochemistry	15–20%
Surface and colloid chemistry	5%
Atomic and molecular structure	10%

It obviously is impossible in a brief space to account adequately for all the cross-referencing which is involved in any statement of course content. For example, calculations from the third law of thermodynamics via heat capacity via spectroscopic data need some incidental yet fundamental appreciation of molecular structure; heterogeneous reaction kinetics and catalysis are bound to include some adsorption topics from surface chemistry, etc.

It seems generally to be agreed that although the list of topics in physical chemistry appears to be the same as it was two decades ago, understanding has been extended in nearly all areas so that the basic two-semester course cannot properly treat each one. One definite trend which appears is an unwillingness to sacrifice the "classical" physical chemistry, (properties of liquids, colligative properties of solutions, etc.) to make room for the expanded thermodynamics. The former are considered essential to the extension of the latter by such means as the Debye-Hückel theory. The consequence of this squeeze seems to be a reluctant simplification of the discussion of colloids

and the allocation of most of the details of atomic and molecular structure to other courses. This is considered necessary to avoid superficiality. However, the pertinent implications of structural theory must be woven into the physical course wherever they are usefully illustrative, (polarities in solutions, possible compound formation in phase diagrams, etc.) It is extremely doubtful whether quantum mechanics need to be mentioned, or whether the Schrödinger equation should find its way into the student's notes, since he cannot possibly use them at this stage of his training. This is not to say that a genuine qualitative appreciation of the successes peculiar to the quantum-mechanical approach, (such as resonance) can be overlooked. Developing what may be termed a "feeling for physical phenomena" is more important than is overwhelming the college junior with rigorous, elegant mathematics. It often happens in the colleges that the physical chemistry professor also has the students in a course labeled "advanced inorganic" or "seminar" in which he is able to insure their familiarity with structural theory. The same result is achieved by university curricula which offer a third term of physical for the undergraduate.

PEDAGOGY

There are a set of pedagogic puzzles which are rather unique to the presentation of physical chemistry. Several already have been indicated. Foremost among these is the problem of adequately emphasizing the phenomenological background to thermodynamics. Some assistance in this may be gained from a brief, qualitative, yet carefully accurate presentation of the statistical basis for thermodynamics. Certainly the order-disorder aspect of entropy is useful. Another psychological advantage is gained by mixing the more familiar such as refrigerators with the more abstract such as Carnot engines. Another puzzle always arises in trying to establish the degree to which a treatment will be rigorous without being exhaustively out of balance. The danger of oversimplification which may require unlearning in later courses is an ever-present one in physical chemistry. Illustrative problems which yield good answers via convenient quadratic equations are all too atypical of those arising in research. The student should at least be led to realize that electronic calculators get fed chemical problems.

The use of a large number of assigned problems contributes much to the notoriety among students of physical as a "tough" course. This arises not because the mathematics goes beyond simple proportion, but because solution of the problem is seldom an end in itself. Carefully chosen problems require the student to begin to use mathematics as a means of expression, much as he is already familiar with using words. The facility and validity of graphical methods of evaluation are invariably a new realization. Exercises in tossing data into formulas seldom accomplish this. Professors agree that teaching students to solve problems is one of the most complicated aspects of teaching physical chemistry. A variety of techniques are employed. Problems always loom large on physical chemistry examinations. They are there less to test the student's accuracy with his slide rule or the proficiency of his memory for formulas than they are to help to teach him to devise methods of obtaining solutions. Open-book (or even open-library) examinations are popular but never used exclusively. Often the size of the class and the press of other duties force the professor unwillingly to compromise both the number and type of problem assignment. Some use is made of quiz-section techniques as familiarly practiced in the conduct of other courses. Even here, such a meeting with the students cannot take the form of a drill session but must point more toward consultations with individuals. College professors with smaller classes (usually 12 to 20) are more fortunate in being able to abandon the formality of lecturing in favor of a more intimate discussion, sometimes with the students solving illustrative problems in class.

It is in this connection that the conduct of the basic physical chemistry course in colleges differs significantly from that of the larger university classes. Whereas the university undergraduate sees research going on all around his classroom, the college professor is obliged to develop in his students an appreciation of research activity by more vicarious methods. One of the most effective of these is to assign problems which expect the student to get beyond his textbook and into the original literature. Problems which require the student to search out and critically organize his data can be devised to illustrate nearly all topics in physical chemistry. The use of microchemical data to yield electrode-potential information on the transuranium elements is one example. Another is to have the student become familiar with the various tabulations of thermodynamic data published by the National Bureau of Standards. Another successful scheme for promoting acquaintance with the current literature is to choose data for examination problems from recently published articles. The reference instead of the answer is supplied the student. He is usually agreeably reassured to discover that at least something in the current literature is not so formidable as to be beyond the interest or the capabilities of an undergraduate.

PHYSICAL CHEMISTRY LABORATORY

In contrast to the virtual standardization of course content, there appears to be wide diversity in the conduct of the physical chemistry laboratory in different institutions. Most notable is the variation in the extent of collaboration between the classroom and laboratory work. This varies all the way from that in the well-equipped small college where each member of a small class does the same experiment coincident with the topic's discussion in class, to that in institutions where the laboratory work is a completely separate course, not even taken concurrently with the lecture work. There do, however, seem to be some definite

trends becoming established in the conduct of the laboratory work. Most pronounced is a desire to get away from isolated manipulative exercises typical of a course in instrumentation. Another is to get students to analyze their data in terms of errors which are inherent in method and technique. A third is the demand for a carefully prepared report, accurately stating a record and evaluation of the experimental data and derived quantities.

The first of these, organizing the work into units or projects, is accomplished in several ways. Some courses are built around the use of an "unknown"; the student is expected to make a reasonably exhaustive study of all its physical properties and derived thermodynamics calculations. Few courses supply the student with complete detailed instructions for obtaining and fitting together this data. The same is true of the degree to which the apparatus is assembled ready for his use. The balance between time saved and technique learned suggests that sometime during the course the student should be confronted with an unassembled potentiometer circuit and a laboratory problem for which his directions do not anticipate all the mistakes he may make. One institution training both engineers and chemists assigns the latter to work with groups of the former on unit-process projects. The chemists are expected to make the necessary physico-chemical measurements to supply the engineers with the data upon which the unit process can be designed.

The style and type of laboratory report required of physical chemistry students is by no means uniform, yet nearly all professors agree that an attempt should be made to have it include a critical analysis of errors. This is seldom involved in the laboratory reports required in other courses. The emphasis should be not only on consistency and accuracy of observation but the student should also be made to realize the quantitative effect of limitations in technique and inherent assumptions of method on the derived result. This again focuses his attention on the very practical aspects of graphical representation of data. The diameter of the circle representing his observed point and the care with which he constructs the slope tangent to the curve at a point gain a new significance. Neatness and clarity are not merely desirable; they become necessary.

Another direction in which the physical chemistry laboratory contributes to the maturity of the student chemist is in acquainting him with the standard and specialized compilations of data which allow him to check the reliability of his results. He should be urged to look beyond his text and manual or even his handbook and to include a properly documented comparison in his report. Some institutions adopt the interesting variation of having each student prepare the report on one experiment during the year in the form of an article to be submitted to an appropriate research journal for publication.

CONCLUSION

There are two candid admissions made by most professors of physical chemistry. The first is that they thought it was a "tough" course when they took it as students. The second is that they expect their students to cover considerably more ground than they themselves did as undergraduates. If the foregoing comments have been able to summarize the current attempts to resolve this paradox they have served their purpose. The author's sincere appreciation goes to those whose solicited opinions and comments were so helpful in shaping this discussion.

GAS-COLLECTION BOTTLES

EARL SHUMAKER

Kent State University, Kent, Ohio

In our freshman laboratories we have the problem of getting all the equipment the student is going to need into one drawer. We have partially solved this problem by having the gas-collecting bottles used as common property in the different laboratory periods.

We have set aside one drawer in each row of desks for their storage. This allows four bottles for each student, or a total of 24 bottles in each row of desks. For the sake of neatness, we have had racks made of half-inch plywood, which gives a greater strength than a half-inch board. Holes for the bottles to stand in are cut with a Pawood circle cutter in a post drill.

We have found that we are not only saving space badly needed by each individual student, but we are also cutting down materially on investment in gas-collection bottles.

RECOLLECTIONS OF THREE GREAT LABORATORIES

GEORGE JAFFÉ
Louisiana State University, Baton Rouge, Louisiana

SINCE I have had the good fortune of knowing a comparatively large number of great scientists and since I enjoyed the privilege of working in three of the most famous research schools (those of Ostwald in Leipzig, of J. J. Thomson in Cambridge, England, and of the Curies in Paris), I have been asked quite frequently to give talks on the relationships which I have had with these famous scientists. This I have done, before sections of the Society of Sigma Xi, of the American Chemical Society, and of various other scientific and students' groups. Upon several of these occasions people whom I considered competent to judge have encouraged me to publish my address. This astonished me at first because I thought that my recollections could not be of much interest except by being personally conveyed to a suitably selected group. Finally, however, I have been convinced that the interest which my talks have aroused was mainly due to the rather unusual number of relationships which I have had with great scientists of the generation before mine.

This is the reason for writing the following report and it also accounts for the personal form which I have given it. The relation between two people is—mathematically speaking—a function of two variables, even if the one of them is a "capital A" (and eventually a great man), and the other a "little x." It would be a vain hope to aim at objectivity where the relation itself is the gist of the matter and its only possible merit is to have been justly related.

Scientists are bred, or should be bred anyway, to talk as little as possible in the first person. There are exceptions to this rule. Let me quote an illustrious example. Helmholtz was certainly one of the most objective of scientists, and he was also one of the most modest of men. When his 70th birthday was celebrated, however, he delivered at the banquet an address on his most personal experiences as a scientist. This address is in my opinion one of the greatest *documents humains* left to us by a great investigator, and I welcome the occasion of recommending it to the readers of these pages.

Now, I am very far indeed from drawing any kind of parallel between Helmholtz and myself, but there is no reason why somebody may not learn from another man whose stature is far above his—that is exactly what he should do. What I have learned from Helmholtz in this regard is the following fact: that it may not be unbecoming, even for a scientist, once in a while, and particularly when he attains the age of a patriarch, to speak out as a man in the first person and let conventions be conventions.

If this attitude is accepted—and the reader will have no means of preventing me from accepting it—I am bound to say a little more about myself than exactly pertains to the characterization of the three schools of research. A conscientious observer will have to mention the kind of photographic apparatus and the nature of the objective which he uses in photographing his objects, and this is the explanation and apology for my saying a few words about my own background.

PERSONAL BACKGROUND

I was born the son of a German father and of an American mother in the city of Moscow (Russia). This rather motley origin may account in part for the unusual training which I had gone through later. Early in life I became a passionate traveler and a lover of languages (what the Autocrat called a 'verbivorous plant'). I have never been, however, a Russian subject. My being born in Czarist Russia was, so to speak, a geographic accident as my father did business in Russia at that time. He never meant for his children to become Russians, not even by training, and so he moved back to Germany when they reached school age.

My schooling was not quite regular from the German point of view, as I had only one year of preparatory school (instead of three). I then passed through one year of a Real gymnasium, and eight years of Humanistisches Gymnasium in Hamburg. There I had ample occasion to feed my linguistic inclinations; we were taught not only French and English, but had nine years of Latin and six years of Greek. Many modern-minded educators have exclaimed, and partly in violent language, against such a "waste of time." I mention this here because I am of the opposed opinion. There are few things in life from which I have derived so much information and pleasure as from my training in the ancient languages, and if it is argued that they should not be taught in high school, because their knowledge is not "useful," I feel inclined to retort: "that is one reason more for teaching them."

Having passed, in 1898, my *Abiturientenexamen*, i. e., the examination that concludes the gymnasium and opens *any* institution of higher learning to the student, I went to the University of Munich for one year. In this connection I would like to mention that students in Germany change from one university to another much more frequently than they do over here, even during their undergraduate training. This is, in my opinion, a habit which should be encouraged, by simplification of the method of transferring credits, because it certainly

broadens the mind of the student to pursue his curriculum under different teachers.

I had definitely set my mind on studying mathematics and the natural sciences, with the then modern science of physical chemistry as my major field, and I had chosen Munich, the "Athens on the Isar," because I was greatly interested in music and the fine arts. I devoted much time to the Muses, here and later. Again I must confess that I do not regret this "waste of time" though I had to give up attending the physics laboratory on account of those distractions. As a matter of fact, though I became a physicist, I never went through a laboratory training in physics. I had ample occasion to make up for that when I taught physics laboratory myself. This proves my point that you will learn what you need whenever you need it, but you will hardly ever learn what you do not need unless you learn it at school.

Of my academic year in Munich and of the famous teachers whose classes I attended there I would like to mention only one little anecdote concerning the chemist von Baeyer. When he came to speak of cacodyl, he told his class that having synthesized this substance he did "what every chemist would have done, i.e., he smelled at it." Naturally, out he went, and had not someone come by, "there would have been a chemist less."

It is not without intention that I tell this little story, because I want to attach to it a remark on a difference between the American and European system of teaching. In all German universities the elementary courses in all fields are given by the major professors. This seems reasonable since the elementary courses are the most difficult ones to teach and require the most of teaching experience. This is not the only reason for advocating the continental system. All students—and particularly those who take this course with practical purposes in mind—should be confronted once in their lives with the true spirit of scientific research, its risks and rewards. Now I ask anybody who has devoted but the slightest thought to higher teaching whether the object just mentioned is better achieved by a man who can tell stories like that told by von Baeyer, or by a youngster—though possibly a future von Baeyer—who has struggled through the material in the assigned textbook the night before? It is different for advanced, and particularly for very advanced courses. There an experienced lecturer may easily speak "above the heads of his hearers" having learned to hide the intrinsic difficulties by elegance of technique and smoothness of delivery, whereas a younger man may help the (more mature!) students by letting them share in his own difficulties and perplexities.

I had planned to spend my second academic year in Leipzig. I had chosen this university because Ostwald was teaching there and, at the same time, because Leipzig was one of the musical centers of Germany. I did not intend to take my Ph.D. degree there, however, but rather to go to Göttingen after my second year and work with Nernst with whom arrangements to this effect had been made. I did not realize at that time that I would become so captivated by the fascination of Ostwald's personality as to stay in Leipzig and take my degree with Ostwald, a little fact—unimportant as it is—which Nernst never forgot.

OSTWALD

When I entered Ostwald's laboratory in 1899 it was at the height of its development and fame. The years of militancy when physical chemistry struggled for recognition had passed. The followers of the theory of dissociation were not ridiculed anymore as "Ionians," but were looked upon as rather useful people, even by industry, which is the ultimate form of recognition. As a consequence of this situation the laboratory was as crowded as it could be.

The previous years of struggle, in a narrow and old-fashioned locality, may have represented a still more brilliant epoch in the history of the laboratory, with a closer contact between the master and his pupils. Now he had built, only three years before, a splendid new laboratory, constructed and equipped entirely according to his own ideas, and the distance between the head and the members had become longer.

There was room for about 40 people who did research, and every bit of space was occupied. I never saw a more cosmopolitan school; in the course of time there were students and graduates from practically all countries. I am not going to bore the reader with a list of names; it is well enough known that in those days every physico-chemist of any renown had gone through Ostwald's school. It was said in jest that his assistants had forgotten their German but had not yet learned any other language. The most famous of Ostwald's American pupils, Richards, Noyes, and Lewis, belonged to earlier crops, but there were always quite a number of them in my time.

It may be interesting to state what means had been contrived to conduct research on such a vast scale and still make all researchers imbibe the spirit of one man. First of all there were the assistants who acted as liaison officers and had to be mature scientists. In my days, Luther, Bredig, and Bodenstein, all of them well-known men in science, conducted most of the research work directly. Still, every single problem was given by Ostwald, or more correctly, was chosen after consultation with him. It was a favorite idea of his that young researchers should choose their subject themselves. "Well," he would say, "you have attended my lectures, so lots of problems must have occurred to you." (By the way, a very remarkable hint that lectures should not only convey what is known but also problems that ought to be attacked.)

May I digress a little and remark that Ostwald held very radical views on education. He was very much opposed to the humanistic training which was prevalent in his day and fought all his life for doing away with Greek and even Latin from most high schools. In this, I am afraid, he was a little too successful.

Apart from this opposition he was very heretic regarding the value of examinations. I remember

listening as a young student to a conversation between him and Wislicenus wherein both expressed the opinion that nobody is likely to become anything in life who had not lost at least a year at high school. It had taken Ostwald himself seven years to get through five years of his Russian high school. I must confess that I felt very much depressed because I had never succeeded in losing a year at the gymnasium.

If I reconsider the matter now, I am afraid that those two great chemists were not altogether wrong. A boy who is too much adaptable to any system of training is not likely to go far in original ways. Of course, and this is essential, it must be asked why a youngster "flunks" at school. If it is simply on the grounds of imbecility or idleness, that would not improve his horoscope. But if it is because his gifts or interests are too strong in some regards, and therefore too weak in others, this changes the aspect. Ostwald himself was a poor pupil in high school because his predilection for chemistry was extremely strong in very young years. Making fireworks under the desk in school, or even in mother's kitchen, usually does not improve the school record, though it may lay the foundations for a great chemist. However, our educational system must not be based on the needs of future great men. They become what they are, not in virtue of the system through which they are made to pass, but in the teeth of any system, and educators must strive to keep the average high. Ostwald was the most versatile man I ever met, and his later career, of which I am going to report, proves the point. His fellow-students had prophesied that he would become nothing on account of his "damned versatility," and he certainly was not the type for which to model an educational system.

To return to the question of how one man could conduct such a large school of research I must mention a most important institution. There was a weekly seminar: "Reports on scientific work," meaning not reports on scientific work that had been published in the literature, but the scientific work that was going on in the laboratory itself. Every piece of research was treated there more than once. The first time a topic was presented Ostwald himself, or one of the assistants, exposed the line that was to be followed. The second time, as a rule when the research was well under way, the researcher himself reported on his work. In this stage he mostly had to expose the difficulties which he had met. Finally, a report on the finished paper was given.

This institution seems to me to offer very great advantages. First of all, Ostwald himself could exert his influence even on investigations which he did not conduct himself, though of course the system presupposes a leader of Ostwald's extraordinary memory and quickness of perception. Secondly, everybody knew what everybody else was doing. There was no affectation of mysteriousness in the place, rather a spirit of what Ostwald called "brotherly openness."

Naturally his criticisms were somewhat feared, especially as they occurred *coram populo*. His main object was to develop originality of thought and independence of judgment. For this reason he wanted everybody to choose his own subject, and insisted that everybody go to the workshop (a special students' workshop) to build his own apparatus. He was a friend of what he called the cork-cardboard-pin system, which is so utterly antiquated by now.

He did not like research which went too smoothly. It is reported that in one instance, when a student reported on a very neat piece of research in kinetics, he remarked, "That's all very nice, but the trouble is that it fits the theory too well." And in another case, where the student excused himself for not having been able to find the desired point, he said: "My dear young man, that is the best thing about your paper."

The extraordinary influence and success of Ostwald as a teacher seems to me traceable to two facts: the astonishing facility with which he produced new ideas, even in conversation, and the art of handling young people according to their individuality. I hope I will be pardoned for giving, instead of generalities, the experience of one pupil—I mean myself. As Ostwald knew of my interest in music, this is the way he advised me when the time had come to write my thesis: "You must write a paper as Beethoven wrote a symphony. Think of the fifth symphony; in the second movement, just before the end, he gives the second subject an entirely new and most impressive turn. That is what you ought to do also. When you have investigated a room, before you finish the description, open the window and show the people the kind of landscape it leads into." A very good piece of advice, but I doubt whether Beethoven is any easier to follow than Ostwald: you must have riches in store. However, here you see the first-class teacher at work. He knows how to impress the young man in a way that he will not forget, not even the words, in almost 50 years.

THREE PERSONALITIES

Besides being a member of the great school of research of which I have been trying to give the reader an idea, I had the good fortune of becoming a friend of Ostwald and his whole family. I wish I could tell you that I attracted his attention by my scientific achievements, but that was not the case. I mentioned before that I had not intended to graduate in Leipzig, and if I became a pupil of Ostwald's—his last personal pupil, I think—it was due to the fact that I could play the violin. Ostwald was a very good musician himself. He played several instruments and had been particularly fond of playing quartets in his early days. In the meantime his children had grown up, and he had three quarters of a string quartet in his family. Now I was able to provide the missing fourth, and thus it happened that I fiddled my way into science.

This little circumstance proved of some importance to me. Ostwald was very proud of his family quartet and each time a distinguished guest came, he had to listen to us, whether he cared for it or not. It is by this accident that I learned to know quite a number of famous scien-

tists, amongst them the two other members of the illustrious triumvirate that had founded modern physical chemistry, namely: van't Hoff and Arrhenius. All three of them were very close friends and their friendship was of old standing. Van't Hoff, one year Ostwald's senior, was his admired leader, and Arrhenius, seven years younger than Ostwald, had been his first foreign research student, in 1884, when Ostwald was still a professor in Riga. Thus they were frequent guests in Leipzig.

It is difficult to imagine three men so intimately connected, and yet so different, in type as well as in achievements. As for their external appearance, van't Hoff was a slender, most distinguished looking Dutchman. Arrhenius, the Swede, was of a more stocky kind. He did not look like a scientist at all, but rather like an industrialist, say a beer-brewer. Finally, Ostwald, the German from the Baltic Provinces, had the appearance of a strong peasant, though his blue eyes of extraordinary lucidity and his red beard, mingled with grey in my time, singled him out in almost any surroundings.

The typology of great men has not been written yet. Still, Ostwald himself made a start even in that direction. In his book "Great Men," besides giving most interesting and spiritual accounts of some of the great men of science, he attempts to deduce some general rules regarding their types and the character of their productivity. In doing so he distinguishes two entirely different types, the "classic" and the "romantic." Now this distinction—borrowed from literature and the arts—is hardly very original. What is entirely original and characteristic for Ostwald (who had a theory ready for everything) is the psychological reason he gives for the difference between the classic and romantic types. For him, the physical chemist, this is merely a question of rate of reaction. The classic is a man of slow rate of reaction who aims at utmost depth, the romantic is the man of great speed of reaction whose activity tends into breadth. He is explosively full of ideas, his rate of production is so high that he often does not find the time to develop an idea conclusively before the next problem attracts—or distracts him, whereas the classic lets things mature and would not publish anything until he has entirely satisfied his own severe standards. It is hardly necessary to add that it is the romantic who becomes influential as the head of a great school, whereas the perfect classic rarely has pupils at all.

Now, there can be no doubt that according to this definition, van't Hoff was a full-fledged classic. All his work has stood the test of time, and there is nothing to be added to what he left finished. Ostwald, on the other hand, surely must be called a romantic in his own sense: a man full of ideas and suggestions, explosively charged so that he gave sparks when you got near him.

If van't Hoff was a classic, and Ostwald a romantic, what about Arrhenius? Well, there is the proof of the deficiency of any typology that will admit only two types. Nature is more inventive than the human mind which endeavors to restrict her to its categories. Ostwald would certainly have called Arrhenius a classic, but the latter was very different from both characterized types: rather slow in reaction and talk but, as is well known, full of fantasy in his conception.

Not only were these three Nobel laureates of early vintage different in character, but also different in their significance for the history of science. I believe I am not wrong if I assert that van't Hoff was the greatest scientific genius amongst them. He created the new ideas and saw the new ways of posing problems in chemistry. Next to him ranks Arrhenius, as far as I can see, for the number and importance of his new concepts. However, if they were, so to speak, the brains of the movement, Ostwald was its arm, or, to use a more modern metaphor, its minister of propaganda. I do not mean to belittle the value of his original research, but I believe his conspicuous role in the history of science is that of early recognizing and then propagating a new science. He became something new and modern in science: the organizer of a branch of it. *He* wrote the first big treatise, a formidable work of tremendous influence; *he* founded the first journal of physical chemistry; *he* did most of the teaching until a second generation had grown; and, last but not least, *he* did the fighting for the new ideas in journals, at meetings, and even in the stagnant old academies. And how did he do it! In such a way that, within ten years, physical chemistry was as recognized a science as her older sisters inorganic and organic chemistry, and that his laboratory became the school for the world in this new science.

In this connection it may be of interest to mention that Ostwald was also one of the first to recognize the importance of the work of Willard Gibbs. He excavated Gibbs' big memoirs from the *Transactions of the Connecticut Academy* where they lay buried, and translated them into German. From that moment began the ever rising influence of Gibbs' ideas.

It is most interesting to note that van't Hoff, Ostwald, and Arrhenius had to face exactly the same problem in those years, a problem that arises frequently for scientists of their age, say between 50 and 60, and to which again Ostwald has drawn attention in general. I am speaking of the fact that scientists at that age are apt to become exhausted, especially if their productivity has been unusual. It also appears that this problem has gained in importance in our rapidly living and highly competitive age.

Those three men reacted in a very different way, characteristic for each of them. Van't Hoff, the distinguished and retired "classic," chose the most inconspicuous solution, that of resignation. He did not become inactive. For quite a number of years he published, together with some pupils, that long series of papers on oceanic deposits. Valuable in themselves they show a van't Hoff grown tired, living on one of his former ideas and expanding it indefinitely.

Arrhenius' solution exhibits less of resignation. He gave up chemistry, but he sought and found a field where he could apply his vast knowledge and his great imagination to the propagation and popularization of

scientific problems on the largest scale. I am speaking of his application of physical chemistry to cosmological problems, of his book "Worlds in the Making" and others which were of very great influence before Eddington and Jeans took his place in similar lines.

If we now come to Ostwald, there is no room for resignation in a protean mind like his. When he realized that physical chemistry could do without him and when he avowedly felt exhausted after an almost superhuman amount of work—it has been computed that at least 22 volumes representing about 16,000 pages had been written by him—at that moment he entirely lost his interest in chemistry. His energy and his sense of enterprise remaining the same, however, he looked out for other and more comprehensive fields of activity. First of all he became a philosopher. In 1902 he published his "Lectures on Natural Philosophy"; about the same time he founded the *Annals of Natural Philosophy* and later became the head of the big monistic movement in Germany. He always had been a painter; now his interest in painting led him to make his own colors, and from this source derived his theory of colors, theoretically and practically a tremendous enterprise which has been of far reaching influence even in the arts and industry. Furthermore he became interested in a world language and propagated one of them, Ido, with his usual energy. He fought for standardizing the shape of all publications in a form dictated by the golden section. There is no end to all his activities, particularly after his premature retirement from his professorship in 1906. Here we see the "romantic" in his retirement.

TRAGEDY OF BOLTZMANN

Before inviting my reader to leave Leipzig with me and go to Cambridge, I have to mention another great scientist, a very great scientist indeed, whose pupil I was in Leipzig and with whom I had the good fortune of becoming well acquainted. I am referring to Boltzmann.

In order to give this part of my narrative the adequate background I have to say a few words on the general situation in the scientific world in those times. Around the year 1900 a violent war was going on between the two scientific trends which were referred to as that of "energetics" and that of "atomistics." Ostwald was the head of the first of them and, as will be understood from what has been said before, a very aggressive head. He and his followers claimed to be representatives of what they called somewhat boldly "science without suppositions" (*voraussetzungslose Wissenschaft*). They demanded that physics and chemistry, and even a deductive science like mechanics, had to be purely descriptive (phenomenalistic). The description should be based on a few entirely general principles such as the two laws of thermodynamics. The principles should correlate observable data only, and theories should not make use of detailed models. "Thou shalt not make unto thee any graven image" was a metaphor used by Ostwald in this connection.

It is astonishing that he, a chemist, claimed the existence of atoms not to be proved by the stoichiometric facts. As late as 1904, in a Lecture to the Royal Institution, he tried to explain Dalton's laws of constant and multiple proportions as equilibrium effects which are far from making the atomistic constitution an experimental fact. Consistently he did not consider the consequences of the kinetic theory of gases as conclusive, since they are based on very specialized and simplified models, not to speak of the abstruse idea of considering the second law of thermodynamics a theorem in probability.

On the other side stood the followers of atomistics who in those days were fighting somewhat on the defensive. They had, of course, to acknowledge that their constructions were mere models but they defended the legitimacy of such models and contended that a deeper insight into the laws of nature could not be obtained by just joining together observable facts, but had to be gained by deducing them from definite concepts regarding the constitution of matter. The head of this group was Boltzmann, the only survivor of the three great theoreticians who had created the kinetic theory, Clausius, Maxwell, and Boltzmann.

Though Ostwald and Boltzmann were rather violent opponents scientifically they had the highest esteem for each other and were good friends personally. When a chair of theoretical physics was to be founded at the University of Leipzig, Ostwald was broadminded enough to insist that the best man available was to be called, and that was Boltzmann. Thus it came that students in those days could see the two heads of opposed schools teach amicably side by side and discuss their divergent ideas in the same university and even in the same seminar. Naturally the students of either of them were their henchmen, and so it happened as it did before Troy: while the gods fought in the skies, the simple men fought on the earth, with even more violence but less sound arguments.

If I judge Ostwald correctly, he must have had in mind, to "fight it out" somehow or other, and even to convince Boltzmann of the correctness of his, Ostwald's, ideas. At first things seemed to go in this direction: Ostwald appeared to have the better of the argument. This, I believe, was due to two reasons, first to the difference in character of the two men, and then to the fact that Boltzmann just then was passing through a stage of mental depression.

As I have mentioned already, Ostwald was a great fighter who had often carried a whole assembly of scientists, almost against their own will, to some aim of his. Boltzmann on the other hand, though a man of big bulk, with a formidable black beard, was a rather soft and vulnerable character. "He was not of this world," Ostwald writes of him, and somewhere he states himself that without Mozart and Schiller there would have been a man with his shape of beard but not he. This remark is characteristic in a double sense: first, because it indicates the influence music and poetry had in the life of this representative of pure science, and then by the very choice of the names Mozart and

Schiller, not Beethoven and Goethe. I know from personal experience how delicately this bulky man played Mozart, with stout and clumsy looking hands, much like the famous French pianist Pugno whom he resembled in external appearance.

Though Boltzmann was able to wage quite a sharp pen in a written controversy he was not a debater like Ostwald being somewhat handicapped by a rather feeble and high-pitched voice which contrasted with his exterior. He must have felt that he was on the defensive and was somehow overshadowed by the brilliancy of his friend and opponent; at least so it seemed to us who were in the camp of energetics. As a matter of fact Boltzmann was very unhappy in Leipzig. He was homesick and was longing for his native Austrian mountains. His depressive mood must have reached a first climax for he made an attempt at his own life while he was in Leipzig. He stayed there only two years altogether and then returned to Austria where a professorship at the Vienna University was offered to him. There, as is well known, he ended his life by suicide about four years later.

I do not venture to guess what reasons led a scientist of his caliber to give up his life after what really was a wonderfully successful career. I cannot help feeling, however, that the scientific situation which I have tried to sketch was not altogether disconnected from his resolution. Given a depressed disposition he must have felt in Leipzig that the development of science was not going in that direction for which he had fought all his life. No doubt he felt dejected at the fact that ideas, the superiority of which he surely felt himself, were being overshadowed. Boltzmann's death is one of the truly tragic events in the history of science, like the decapitation of Lavoisier, the commitment of R. J. Mayer to a lunatic asylum, and the crushing of Pierre Curie under the wheels of a truck. It is all the more tragic as it happened on the very eve of the final victory of his ideas, brought about by the investigations of Svedberg and Perrin on the Brownian movement and by the whole modern development. Ostwald himself lived to recognize the existence of atoms as experimental facts, though they have ceased to be the simple indivisible units the ancient philosophers dreamed of.

THOMSON

After I had taken my Ph.D. degree with Ostwald, early in 1903, I did not yet enter a professional career. It is pretty evident from what I have mentioned regarding my background and inclinations that I wished to continue my studies and do post-doctoral research *abroad*. The choice that presented itself to me at that time was either go to London with a recommendation of Ostwald and work with Sir William Ramsay or to go to Cambridge with a recommendation of Boltzmann and work with J. J. Thomson. I chose the latter alternative, and the decision was not only of importance for my personal career but also appears to be characteristic of the moment at which it was made. If I, a kind of miniature Hercules, chose the path of physics rather than that of chemistry, though I had been a personal pupil of Ostwald and could expect of him all possible furtherance, it must have been clear to me that physics, at that moment, represented the more startling enterprise. And I was right about that.

The name of J. J. Thomson—later Sir J. J. Thomson, or simply "J. J." as his students called him—has an almost magic sound to the ear of a physicist, so much is it connected with the extraordinary development of modern physics. I think it is no misstatement if I claim that about the year 1900, or a little earlier, physical chemistry turned over to one of her ancestors, physics, the role of being the most revolutionary and most rapidly progressing of the exact sciences. In this development the Cavendish Laboratory and its spiritual head held the same position which had been held by Ostwald and his laboratory in physical chemistry. Of the three great experimental discoveries which inaugurated the modern era of physics, that of X-rays, of the electron, and of radioactivity, the discovery of the electron was due to J. J. Thomson himself. In addition, it is no exaggeration to state that most of the basic work of establishing the modern viewpoint, especially concerning the problems of conduction of electricity through gases, was done in the Cavendish Laboratory.

Therefore, this laboratory occupied an almost unique position and my expectations were correspondingly high when I changed from Ostwald's to J. J. Thomson's research school. I must confess that my first impression of the laboratory itself was a little disappointing. Coming from a modern institution I found the famous school lodged in a somewhat old-fashioned building with bare brick interior walls and not quite corresponding to the standards of neatness to which I had been accustomed. This reminds me of a saying of Ostwald's that some of the neatest work is done in the dirtiest laboratories, and characteristically he immediately gave an explanation, something of this kind, "In clean places you rely too much on the cleanliness, otherwise you take your precautions." I believe he had Pasteur in mind whose work he greatly admired and, by the way, translated in part into German.

It is superfluous to say that I was disappointed only with regard to the external appearance of the laboratory. With respect to the internal scientific life and activity the "Cavendish" would have been up to any expectations. Let me first try to characterize the *spiritus rector* of the place and then the school he stood for.

There can hardly be a greater difference in type than there was between Ostwald and J. J. Thomson. If Ostwald was the most versatile scientist whom I ever met, J. J. Thomson was the most concentrated, one might almost say the most one-sided, provided this ominous term is applicable to his superior class. I do not mean to imply that J. J. Thomson was narrow-minded. Quite to the contrary, he was interested in everything and had an open eye for everything. When I saw him for the last time at the age of almost 80 his conversation was as lively and sparkling as ever before,

and whoever reads his "Recollections and Reflexions," written at the age of 80, will see what a good observer and unprejudiced critic of men and things he was. His career, however, was as undeviated as it could be by anything beyond his professional field. He did not "waste time" as a student, as Ostwald did, in writing poetry or music, nor did he transgress into philosophy at a later time. Mathematics and physics, physics and mathematics, and just as much sport as was required to keep the body in good shape: that was his formula.

It was this intentness of purpose which brought J. J. Thomson to the chair of Maxwell and Lord Rayleigh at the age of 28, which raised him finally to the presidency of the Royal Society and to the mastership of Trinity College in Cambridge. This mastership is considered the highest scholastic honor in Britain, and had been always held by clerical men until then. It was this concentration which also accounted for J. J. Thomson's marvellous success at the head of his school, which produced half a dozen Nobel laureates besides himself.

When I came to the Cavendish Laboratory it also had passed its militant stage. In the beginning the discovery of the electron met with little more than doubt, and so did J. J. Thomson's concepts regarding the conduction of electricity through gases. I heard John Zeleny say that he was in Berlin at that time working in the laboratory of Warburg. When the discovery of the electron was announced, nobody in Berlin would believe in it. So he, Zeleny, packed his trunks to go to Cambridge and thus to become the pioneer of the new ideas in this country.

When I entered the Cavendish Laboratory, some seven years later, J. J. Thomson's views were universally accepted and students from all the world flocked together to work with him. There were about the same number of advanced students doing research as there had been in Ostwald's laboratory, say 40. Perhaps fewer nationalities were represented, but there were students from almost all parts of the vast British Empire, besides a few Americans, an Austrian, and myself.

It may be of interest to compare the two great schools and their aims. The scientists who inspired the work were as different as the tendencies and the joint efforts of the pupils. This is perhaps best expressed by a simile. I would like to compare Ostwald's school to a group of explorers who have discovered a new land and who now are bent on spreading out and covering as much ground as possible leaving it to others to complete and intensify their survey. On the other hand, J. J. Thomson's school was like a team of workers who are driving a tunnel through a huge mountain. All efforts are concentrated on one spot, with the corresponding results in depth and penetration. It would not be wise to ask which of the two methods yields the better results. We should be thankful that there are, now and then, great scientists who are able to inspire such team work, whatever their individual method may be. There are no rules for a genius.

The means by which J. J. Thomson carried on the supervision of his school were also somewhat different from those of Ostwald. There was no "internal seminar" as I have mentioned as characteristic of the Leipzig school, though there were, occasionally, meetings of the Cambridge Philosophical Society where finished papers could be presented. However, J. J. Thomson made a tour of the laboratory as far as possible every day, and it is readily understood how eager this procedure made every single one of us to report some progress or to hear his advice if some new difficulty had turned up.

Though the master's suggestions were, of course, most anxiously sought and respected, it is no exaggeration to add that we were all rather afraid he might touch some of our apparatus. I hope I will not be misunderstood. I do not mean to detract anything from J. J. Thomson's fame of being one of the foremost experimental, as well as theoretical, investigators of his time. I report this little detail for two reasons: one of them is to encourage the younger ones of my readers who are aware of not being quite as handy as the handiest; and the second reason is to emphasize the fact that there are different types of hands amongst scientists just as there are different types of spirits. Ostwald's rather boorish hands did not prevent him from being a good musician apart from being an accomplished glassblower and mechanic. J. J. Thomson, however, had "clumsy hands," to use his own expression, in spite of having slender fingers. Probably they were too nervous, and consequently he did the experimenting as a general does the fighting.

Besides his regular rounds, J. J. Thomson assembled his advanced students every afternoon for a cup of tea (we are in England!) in his office. On these occasions "all things, and some others" were discussed—with the exception of physics. Here it was that "J. J.'s" lively conversation, his keen interest in all human affairs, from sport to politics, showed up, and nobody who has had the privilege of joining in these five o'clock teas is likely to forget them.

When I saw the Cavendish Laboratory for the last time in 1933 Lord Rutherford had become its head but the octogenarian Master of Trinity still had his laboratory and his private assistant and was continuing to work. However, the physical plant of the "Cavendish" had changed considerably by more recent additions. The modest old building looked something like Trinity Church in Manhattan in its surroundings of skyscrapers: a venerable monument of bygone times.

THE CURIES

The third research laboratory on which I want to report was very different from the first two: Pierre Curie's laboratory in Paris was a very restricted and secluded place. At that time hardly half a dozen young scientists were working there besides the Curies themselves. Nor was it a teaching laboratory at all, unless you have in mind the highest and most effective form of teaching, that by example. The researchers had, of course, the invaluable advantage of Professor and

Madame's advice—she was *chef de travaux* at that time—but they had to be sufficiently mature to stand on their own feet.

It is one of the happy accidental points in my own life that I was admitted into this select group. Again I wish I could assert that it was due to some scientific achievement, but that would not correspond to the facts. It was by accident.

When Pierre Curie came to Cambridge in 1904 he did not speak any English, nor did J. J. Thomson speak much French. So it came about that I paved my way into the Curie laboratory by acting as an interpreter between those two great physicists. That I was admitted I owe exclusively to the gentle and amiable character of Curie who could not say no to a demand that was put to him under such circumstances. That I could make this demand was most evidently due to the fact that I could express it in French. And this brings me to my point in telling this little story. You young friends of mine who happen to read this report, graduate students and others, do not loathe the fact that we demand from you a "reading knowledge" of French, or German, or even Russian. The day may come when your knowledge proves to be a valuable asset, as in my case. If that does not occur—for such fortunate incidents are rare—the day is bound to come when you will consider that knowledge a valuable asset to yourselves irrespective of its usefulness. And if this day does not come, you do not have the right attitude toward learning.

It is well known that the Curies made their wonderful discovery under the most restricted circumstances. He, though a physicist of fame before he ever met his future consort, had a very modest teaching laboratory in what you would call a city college (he was, as somebody put it, "a miserably poor candidate" which means deficient in recommending himself). The extraordinary feat of extracting far less than one gram of pure radium bromide from two wagon loads of ore was performed in an old, by no means weatherproof, barrack in the yard of the school. Let me add here a little remark which illustrates the relative value of physical and spiritual contributions. The ore was a gift of the Austrian government and consisted of residues which had no commercial value. After the discovery of radium it would have been worth a very big amount of money, but this value it acquired only by the work of the Curies.

The barrack of which I am speaking does not exist anymore, but it ought to remain memorable as the scene of an experience which, I think, is unique in the history of science. It is well known that the ultimate separation of radium from its cousin barium is performed by fractional crystallizations of their salts, a method which the Curies invented but refused to patent, and which is used to this day. At the time in question, the fractionating process was pretty well advanced and the solutions of varying concentrations were arranged along the walls of that modest laboratory. Now, one night, when the Curies entered the barrack they saw, to their immense surprise, all the bowls glowing in a mystic greenish light. The concentration of the mother liquors was so high that they strongly ionized the air and gave rise to a kind of corona effect. It must have been a wonderful experience for the couple, a reward for the huge work which lay behind them.

Finally, in 1904, after the award of the Nobel prize, Pierre Curie became professor at the Sorbonne and obtained a laboratory of his own, but one which was still rather modest and very limited in space. Consequently I, who had been admitted only by the kindness of Curie, had to establish myself on a narrow gallery overlooking a classroom. In spite of this inconvenience, I am mighty proud, as the reader will imagine, to have worked under both Professor and Madame Curie, all the more so as his brilliant career was to end, scarcely a year later, in the known tragic way.

When I worked in the Curie laboratory for the first time I did not see very much of Madame. Her time was occupied to a considerable extent by another part of her human activities: it was the year in which Mademoiselle Eve, the now famous biographer of her mother, was born. However, I had the good fortune of returning to the laboratory as a Carnegie scholar in 1911–12, and then she was director of the laboratory and I saw a good deal of her.

As I have known both Pierre and Marie Curie, and as I have done research under both of them, it may not be out of place to state in what regard they impressed me as being similar, and in what regard as being dissimilar. There have been, and there are, scientific couples who collaborate with great distinction, but there has not been a second union of woman and man who represented both in their own right, a great scientist. Nor would it be possible to find a more distinguished instance where husband and wife with all their mutual admiration and devotion preserved so completely independence of character, in life as well as in science.

The traits which they had in common were those which constituted their philosophy of life and their general attitude to science. Like everybody else who came in contact with them, I was most strongly impressed by their extreme simplicity and modesty together with their extraordinary devotion to their task. It is hardly necessary to add that there was about both of them an unostentatious superiority which was all the more impressive on account of their simplicity. It is well known how much they loathed being distracted by publicity. Pierre Curie wrote to a friend after the excitements which accompanied the award of the Nobel prize in 1903: "I long for calmer days in a quiet place where lectures will be forbidden and newspapermen prosecuted." This was no affectation in either of them. The desire to be left to their task was too dominant in them.

Pierre Curie belonged to that type of creative men who work comparatively slowly allowing their problems time to mature and avoid rather than hasten publication of their results. Thus, in the terminology of Ostwald, Pierre Curie was of the classic type. This disposition made *him* stand aloof when *she* entered upon something

like a romantic enterprise: the search for an unknown element. Of course we cannot know how much his advice and his experience may have furthered her work; however, it is a fact that he continued his own work of different character and did not join her in her field of research until the success seemed assured.

In Pierre Curie's personal behavior calmness and gentleness were the most characteristic traits. Though a man of strong and very liberal convictions he was extremely tolerant of others. I never met a scientist who appeared so much above the strife of opinions and so eager to be just to even the smallest contribution.

And Madame Curie? "He calm, she ardent"—these are the words of her younger daughter and biographer. "Ardent" that is the very word. Those of my readers who are familiar with Mademoiselle Eve's biography of her mother may be interested to know whether my own impression of Madame Curie coincides completely with the picture drawn by the daughter. I cannot confirm this altogether. It is not astonishing, however, that a stranger saw with different eyes than the daughter. Naturally, the latter knew best the maternal side of the great woman and stressed it in her representation. The stranger saw and admired the power of the intellect. He could not know what was going on behind the air of self-constraint or almost impassiveness which became characteristic of Madame Curie after the death of her husband. He would of necessity emphasize what you might call the masculine side of her mentality, her strong temperament and her unusual energy. He would notice that she was almost always the one who led the conversation, even in the company of men like Langevin and Perrin, and that it was not unbecoming.

Though her bearing was very composed and her way of speaking quiet and definite, it was not difficult to notice the flame which was burning within her. How else could we understand this almost incredible ascent: the years of patient (or rather impatient) expectation when she was earning the money for her later studies (hers and her sister's!) as a governess in a gentry home in Poland; then the years of poverty and passionate studies in Paris. It was this flame, the flame of idealism and enthusiasm, that illuminated her way and found her prepared when the great task presented itself.

I certainly do not intend to belittle Madame Curie's intellectual genius when I assert that her achievement in science is almost more astonishing because of the perseverance and the moral strength necessary for the execution rather than because of the ingenuity of the concept. To have started out on the search of an unknown element which could at best be present in a minute quantity within a huge bulk of material, to have carried through this enterprise, one might say against ever increasing odds—this is the wonder of scientific faith almost unique in the history of science.

When I left Paris in 1905 after my first year in the Curie laboratory, I made a trip to the United States. I did not become attached to any institution then but I visited some of the great eastern universities and the Bureau of Standards. I must confess that I feel quite pleased with myself now for having recognized so early the leading part which the United States was assuming, just about then, at the side of the older European countries. After this trip I considered my years of apprenticeship as closed and settled down in my old *alma mater* Leipzig.

It is hardly necessary to point out that a great part of my own research has been influenced strongly by my great masters. Nothing is more inspiring to a beginner than the contact with a teacher who has contributed greatly to his science. It is not the question of how far the youngster may go in his own time. "That is in the hands of the Gods," as Socrates would have said. It is the question of his becoming penetrated by the true spirit of investigation—of seeing its value and fascination—beyond all problems of utility and applicability.

Naturally I realize that not all future scientists, not even many of them, can be as fortunate in this regard as I have been. However, even secondhand reports and the study of biographies may exert an influence similar to personal contact.

Furthermore, the great names which I have been able to mention all belong to history already. It should be realized that Curie and Boltzmann died in 1906, and that Ostwald gave up teaching in the same year. The number of those who have been immediate pupils of these men and who are able to report on their personalities is getting smaller from year to year.

These are the reasons which have prevailed upon me to write this report, and I shall feel amply rewarded if just a few of my readers have become interested enough to study the lives of those great men and that great woman who have been an inspiration not only to me but to generations of scientists.

THE PRESERVATION AND AVAILABILITY OF CHEMICAL KNOWLEDGE[1]

G. MALCOLM DYSON
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There are many virtues in books, but the essential value is the adding of knowledge to our stock by the record of new facts, and better, by the record of intuitions which distribute facts and are the formulas which supersede all histories.

—Emerson

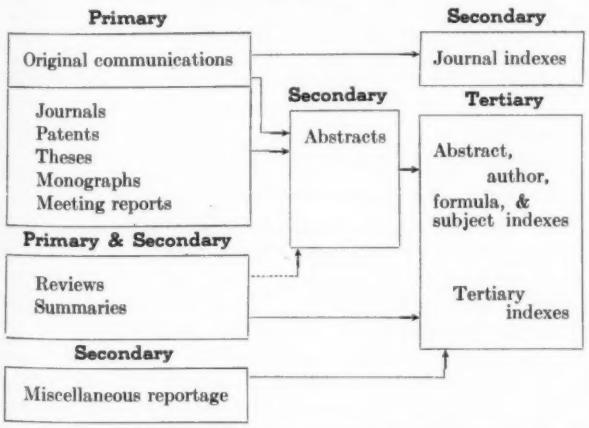
Books are the sepulchres of thought.

—Longfellow

I CHOSE these quotations to head this paper because they epitomize, in more elegant language than I can command, two important aspects of the modern state of knowledge; namely, the essentiality of books to scientific (or, indeed, any other) progress, and the danger that knowledge may become buried in them. Not that it matters that facts are buried, if they may be easily disinterred; but it becomes disaster when they are buried without trace, for it is abundantly evident that unless scientific data are readily accessible they represent only so much wasted time, effort, and repeated waste of these commodities by others.

It is essential, therefore, that much thought and work be given to the problem of making the maximum use of chemical data with the minimum of effort, and the first step in this direction is to inquire into and analyze what is being done about the matter at the moment, and secondly to see whether improvement is desirable and, if so, whether it is feasible.

The present methods of dissemination of chemical information involve a pattern well known to all, and illustrated in the following table.



Original communications are largely the source of primary knowledge, although reviews may include such material. Reviews and summaries are largely secondary publications but their great value lies in the bringing together of widespread facts and theories into a logical pattern, thereby serving to stimulate interest in a given field and to serve as a convenient source for searching. Of these it may perhaps be added that the *Annual Reports* of the Chemical Society and of the Society of Chemical Industry are invaluable examples of what George Eliot described as "a book which hath been culled from the flower of all books."

Primary sources may, of course, be subdivided into journals, patents, theses, monographs, reports of meetings and conferences, but to these may be added a "miscellaneous reportage" in the more ephemeral publications—largely summaries of news and views having a day-to-day interest. This large volume of written and printed matter—the documentary output of chemical science—is a constantly growing treasure house of data. It is occasionally asked whether scientists do not publish too much, but it must not be forgotten that publication is a discipline, and acts as a clarifying influence on the writer's mind, apart from its other uses. Indeed, one has perforce to agree with Blakey:

> The habit of committing our thoughts to writing is a powerful means of expanding the mind and producing a logical and systematic arrangement of our views and opinions. It is this which gives the writer a vast superiority as to the accuracy and extent of his conceptions, over the mere talker. No one can ever hope to know the principles of any art or science thoroughly who does not write as well as read upon the subject.

The most important secondary publications are the abstracts; their nature is sufficiently understood not to necessitate any description here, although I may be permitted to add that they are of great antiquity, Shakespeare himself making reference to:

> Brief abstract and record of tedious days

and also

> They are the abstracts, and brief Chronicles of the time.

[1] Presented at the XII International Congress of Pure and Applied Chemistry, New York, September, 1951.

One of the most important aspects of our literature pattern is the indexes, for if, as Holmes ("Medical Essays," p. 211) says, "Science is the topography of ignorance," then indexes are the signposts to knowledge. They are, perhaps, among the most important documents we possess and a good index well and conscientiously prepared is a considerable contribution to science. Pope, in the "Dunciad," tells,

> How index-learning turns no student pale,
> And holds the eel of science by the tail.

Dr. Johnson adjured Boswell: "I wish you would add an *index rerum*, that when the reader recollects any incident he may easily find it," and in "Troilus and Cressida," Nestor is heard to say:

> And in such indexes, although small pricks
> To their subsequent volumes, there is seen
> The baby figure of the giant mass
> Of things to come at large.

Too much emphasis cannot be laid on good indexing; indeed, it is in the mechanical developments of indexing that we can visualize the major lines of progress in the foreseeable future:

> An index is a necessary implement.... Without this, a large author is but a labyrinth without a clue to direct the readers within. [Fuller: "Worthies of England."]

Fuller wrote about 1640, and indexing has changed but little in principle in the three centuries which have elapsed; but the time is near when, as with other tools, mechanization is imperative.

Clearly, entry can be made by the inquiring mind into this pattern at any point—originals, reports, abstracts, or indexes—and the scientist uses a judicious combination of each. There has been at times a rather snobbish viewpoint promulgated that all scientific reading should be in originals, but this is an extreme view and I believe with Lord Cecil that: "All extremes are error. The reverse of error is not Truth, but error still. Truth lies between these extremes." And the counsel of Emerson on secondary sources is most reassuring, for he said, "I would as soon think of swimming across the Charles River when I wish to go to Boston, as reading all my books in originals."

In any analysis of the operations involved in searching the chemical literature, one factor will inevitably assume great importance, namely, that of time, for Theophrastus, even in his day, held that, "Expense of time is the most costly of all expenses." The two questions which suggest themselves are: (1) Is much time consumed by literature searches? (2) Can it be reduced? The answer to the former is undoubtedly "yes." There are probably not less than 300,000 users of chemical literature throughout the world, each of whom spends, on an average, 200 hours per annum in literature work, making an aggregate of 60,000,000 man-hours devoted to this work. To save even a tithe of this time would be a great achievement. The second question, as to whether it is feasible to save an appreciable portion of this time, depends on an analysis of the factors governing its expenditure. These may be summarized as: (1) Nomenclature difficulties. (2) The intrinsic imperfections of abstracts and indexes. (3) The time taken in the sheer physical duties of consulting books, making notes, and reading material. All this directs our thoughts into three main channels: (1) Can nomenclature difficulties be solved? (2) Can machines be set to work to search existing records, and, if not, how must existing records be altered or supplemented to make them amenable to machine searching? (3) Can abstracting and indexing efficiency be improved? Let us consider each of these in turn.

NOMENCLATURE

The naive answer to this problem is that it only requires agreement. The true answer is that the agreement cannot easily be reached without a more definitive set of rules for nomenclature than those yet available. We can all agree to call $C_{10}H_8$ naphthalene; and $C_{14}H_{10}$ anthracene or phenanthrene according to its structure; but what of

for which the names

Thioxanthene
Thiaxanthene
Dibenzthiopyran
Dibenzpenthiophene
Diphenylene methane sulfide

have been proposed? Or even $CSCl_4$ for which the names

Perchloromethyl mercaptan
Thiocarbonyl tetrachloride
Trichloromethyl sulfur chloride
Tetrachloromethyl thiol
Trichloromethylsulfenyl chloride

have been used during the last 80 years.

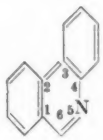
To avoid ambiguities of nomenclature, there must be some fiducial thread running through chemistry relating names, notations, and structures. That this must derive from the structure is obvious; the clearest possible case is therefore to be made out for the one: one correspondence symbolized by:

$$\text{Structure} \equiv \text{notation} \equiv \text{fiducial name} \rightarrow \text{index name}$$

in which structure, notation, and fiducial name are uniquely intraconvertible. More "comfortable" names for general use may be derived from the fiducial names by substitution of a part of their system by trival stems, according to an agreed plan. There is, of course, no intrinsic evil in trivial names—they are often a necessity in common usage. By all means let us have trivial names, but let them be accepted by international consent, and play their proper part in shortening and making more convenient for everyday laboratory use the fiducial nomenclature. In this way the one-one correspondence between struc-

ture and notation will be preserved and ambiguities eliminated. Examples of such a close-knit relation are:

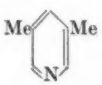
(*a*)



Cipher	$B6_3,1,3.ZN,5$
Fiducial name	1,3-Ternihexalene-5-aza
Probable index name	Phenanthrene-5-aza
Dyson Index	$0.101_39:33.10000.0$

This is based, of course, on the substitution of "phenanthrene" for "1,3-ternihexalene."

(*b*)

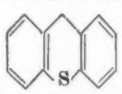


Cipher	B6.ZN.C,3,5
Fiducial name	Hexalene-aza-3,5-dimethyl
Index name	Pyridine-3,5-dimethyl
Dyson Index	0.1079:11.10002.0

(*c*)

Cipher	A64,1-3.C,2,2,4.Q,5
Fiducial name	1,3-Hexatetralane-2,2,4-trimethyl-5-ol
Index name	Pinane-5-ol
Dyson Index	0.11_01_8:1010.3.28

(*d*)



Cipher	$B6_3,1,4.ZS,3.H,3,6$
Fiducial name	1,4-Ternihexalene-3-thia-3,6-dihydro
Index name	Anthracene-3-thia-3,6-dihydro
Dyson Index	0.1001_31_6:32.10000.0

In the last example, if "anthracene" is agreed as the accepted trivial name of the $B6_3,1,4$ operation, then the name will be "Anthracene-3-thia-3,6-dihydro," and this will be used for all official purposes, indexes, abstracts and, in due time, original communications. It is unique in the sense that no alternatives can be formed. It appears in identical order and form in indexes as in textual matter (and so does away with the rearrangement of syllables for indexing) and satisfies Patterson's concept of the essentiality of "a system of good names which can be used in writing and speaking and in subject indexes."

The way is clear for work to go forward to this universal nomenclature of organic chemical compounds which would be so great a boon to all workers in the field.

MACHINES

We must first consider how machines can be applied to our existing records. It can be said without fear of contradiction that books as they are now produced cannot be made directly amenable to machine searching; so we must turn immediately to the second part of the subject, namely, how can the records of chemical science be mechanized?

There are two broad paths available in this connection: (1) the use of micro (or semimicro) records of printed matter, and (2) the use of some other medium for the storage and selection of information.

The use of micro or semimicro records presupposes the existence of printed or written full-scale records and introduces little that is novel. The obvious advantage of these miniature replicas of larger originals is that the book is a form which must persist, and, no matter how successful derived card forms (punched or otherwise) may be, the essential printed record must remain as the parent of the more agile form.

> All that Mankind has done, thought, gained or been, it is lying as in magic preservation in the pages of Books. They are the chosen possessions of man.

So said Carlyle, and it is not to be contemplated—at least for practical purposes—that books will be supplanted, even if they are supplemented, by other forms of record for scientific purposes.

The great disadvantage of the written or printed record in microform is that it must be tied to some form of symbolization for any type of mechanical selection. So we come to the important second part of this inquiry: is there a workable alternative to language for purposes of scientific record or for the manipulation of chemical information? In chemical science there are two distinct divisions of concept, the chemical entities on the one hand, and their formations, properties, and reactions on the other; to which must be added the theories correlating the latter. In other words, chemistry has a series of fiducial points in the structure and composition of its compounds, and, in contrast, in the description of properties and reactions. The fiducial points of structure can readily be translated into symbolical notation (or, through it into an equivalent fixed nomenclature). However, the problem of reactions, preparation, and properties has prompted me to study the semantics of the organic chemist, constituting in effect, an inquiry into the symbolization of concepts. The first attribute of organic chemical writing that forcibly commands attention is the constant repetition of phrases. It appears that, among other things, organic chemists are continually "evaporating to dryness on a water bath," "recrystallizing from alcohol with the aid of decolorizing carbon," "fractionating with a packed column," "filtering at the pump," and "heating under a reflux condenser." In other words, the language of the organic chemist has a comparatively short vocabulary of much-repeated phrases; such a condition is ideal for symbolization, and I therefore ex-

Code for Organic Chemical Language

Operation		1	2	3	4	5	6	7	8	9
Cool	m	cool	in water	in ice	in ice and salt	in CO_2/acetone	in liquid air	internal ice	cool to (−)	running water
Simple Directions	n	place	quickly	remove	as fast as the vigor of the reaction permits	after the reaction subsides	make up volume to (−)	slowly	.....	.....
Addition	p	add	add dropwise	add with cooling	add with shaking	add with stirring	in a current of (−)	at such a rate that the temperature does not rise above (−)	at such a rate that the temperature does not fall below (−)	by the addition of
Distillation	q	distil	distil in steam	distil in vacuum	distil with column	collect fraction	distillate	freshly distilled	in Claisen flask	.....
Solution	r	dissolve	saturate	extract with	digest	pour into water	pour onto ice	solution	tar	residue
Drying	s	dry	(dry) over	dry in desiccator	dry in vacuum oven	dried over Na	dry over anhyd. $CaCl_2$	.....	.....	.....
Time	t	hours	minutes	day	overnight	allow the reaction mixt. to stand overnight	for at least	every	.....	.....
Aggregation	u	powdered	finely divided	large lumps	coarse powder	an intimate mixture of	turnings	pure	wire	foil
Stirring	v	stir	shake	stir vigorously by hand	.....	.....	.....	.....	.....	.....
Heating	w	heat	heat under reflux	heat on H_2O bath	heat on oil-bath at (−)	boil	hot	remove flame when reaction commences	heat to b. p.	remove from flame
Procedures	x	induce	prepare	obtain	transfer	pour	followed by	remove	is prepared by	in sealed tubes
Quantitative	y	yield of produce	increase in wt.	decrease in wt.	excess	produce	crude product	product from previous process	enough	.....
Filtration and Extraction	z	filter	filter at pump with Büchner funnel	filter at pump with sintered glass	fluted filter	filtrate	drain	wash	extract with	.....
Colors	a	white or colorless	yellow	orange	red	green	blue	violet	black	dark
Flasks and Beakers	f	1-liter flask	2-liter flask	3-liter flask	5-liter flask	small flask	small beaker	large beaker	1-liter beaker	5-liter beaker
Fittings	g	mechanical stirrer	3-necked	3-necked equipped with	reflux condenser	thermometer	separating funnel	.....	.....	.....
Fittings	h	funnel	.....	.....	.....	.....	.....	.....	.....	.....
Directions	j	decolorize with carbon	acetylate	precipitate	diazotize	reaction	reaction mixture	when reaction ceases	.....	.....
Directions	k	neutralize	acidify	faint acidity	strong acidity	faint alkalinity	strong alkalinity	neutral	.....	.....
Directions	l	crystallize	crystals	crystallization	for further purification recrystallize from	water	alcohol	benzene	ether	chloroform

"with" = +. Make participle by italicizing, *e. g.*, w = heat; *w* = heating.

perimented on the reduction of the semantics of organic chemistry to sign language.

The table shows an arbitrary experimental coding of organic chemical language which enables the following compressions to be made:

Allow the reaction mixture to stand overnight and pour onto ice; filter at the pump with a Büchner funnel; for further purification decolorize with carbon and recrystallize from alcohol. (142 symbols)

This can be written:[2]

t5r6z2jl46 (10 symbols)

Another example is:

Transfer to a 2-liter 3-necked flask equipped with stirrer, reflux condenser, and thermometer. Heat under reflux, with stirring, on the water bath for eight hours. (132 symbols)

This becomes:

x4→f2g3145/w23+v(8t) (21 symbols)

The volume reduction is 14:1 in the first example and 6:1 in the second. A third and rather longer example is the following preparation of furoin:

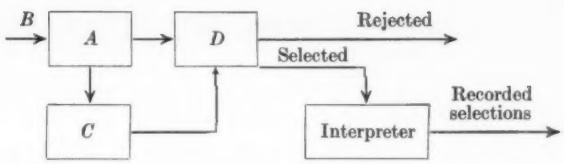
In a 1-liter three-necked flask, equipped with a mechanical stirrer, a reflux condenser, and a separating funnel, place water (400 ml.), freshly distilled furfural (200 g.), and ethanol (150 ml.). Heat the reaction mixture to boiling, remove from the flame, and add dropwise with stirring a solution of potassium cyanide (10 g.) in water (30 ml.) from a separating funnel as rapidly as the vigor of the reaction permits. After the reaction subsides, heat to boiling for 5 minutes. Acidify the reaction mixture with glacial acetic acid and allow to stand overnight in ice. Filter off the dark crystals at the pump and wash with cold water and methanol to remove tar. For further purification recrystallize from methyl alcohol with decolorizing carbon (10 g.). Yield of product 75 g., m. p. 135–6°. (640 symbols)

fg3146nQ(400ml.)q7B5.ZQ.H.C,2.EQ1(200g.)&C_2.Q(150ml.)/j6w89&p25g6r7KCN(10g.)Q(30ml.)n4/n5w8(5t2)k2j6+C_2.Xt5m3/z2a9l2z7+Qx6C.Q→x7r8/14C.Q&j(10g.)/y(m. p.135–6°)(75g.) (165 symbols)

It is not for one moment contended that this is a final arrangement of symbols for organic chemical language, or that it would form an acceptable substitute for language in, for example, journals or abstracts; but such a code can easily be accommodated on an "eight-track" magnetic tape, or by the new card-punching system developed in connection with the Dyson notation, and recently demonstrated in New York. By suitable reading and memory relays the "extenso" version can be recalled and a typed record thereof produced. The principle of such a mechanical method is illustrated in the diagram.

A is a scanner through which the records (card or tape) (*B*) are passed mechanically. Whatever impulse is generated in *A* is compared with a predetermined pattern

[2] Number 1 is omitted where unambiguous; *e. g.*, in this example "j" stands for "j1."



set up in *C*. If a match is secured a device (*D*) is operated which selects the record in question. Cards are selected physically. Series of magnetic impulses may be reproduced in a subsidiary tape. However the selection is made, the selected data are passed through an interpreter capable of reproducing the phrases encoded by an electrically operated typewriter, so that the "extenso" version of the encoded material is finally compiled mechanically.

Since it is implicit in these systems that the symbolization of concepts is independent of language, it follows that the interpreter or translator need not work in the same language as that from which the original records were made. If a sentence,

Tin tetraethyl is prepared by heating in an oil bath to a temperature of 70–80° tinfoil with ethyl iodide in sealed tubes.

becomes encoded to:

Sn.$[C_2]_4$x8w4(70–80°)Snu9+C_2.Ix9

then the memory relays can be arranged to reproduce this as

Sn.$[C_2]_4$ est preparée par chauffant au bain d'huile à une temperature de 70–80° d'etain limaillée avec C_2.I dans des tubes scelles.

which, although perhaps not elegant French, is quite intelligible.

ABSTRACTING AND INDEXING

I do not propose to go into the question of the manner in which abstracting services and indexes can be improved; they have already reached a very high level of performance and anything additional will only be purchased at the cost of enormous financial outlay and effort. Whether this would serve the purposes of chemists generally is a moot point. Indexes have often been criticized on account of their inherent defects, and these, being inherent, cannot of course be exorcised.

The true solution of indexing problems lies in mechanical means; only therein can the potentialities of infinitely variable permutations of concepts be realized. Indexers have often and truly remarked that the normal method of indexing can only produce a single classification, and that by including every cross reference under every potential heading an index would become infinitely long. Machine methods give us the potential convenient use of an infinitely long index with multilateral classification.

THE DUVEEN LIBRARY

SAMUEL A. IVES and AARON J. IHDE
University of Wisconsin, Madison, Wisconsin

THE Duveen Library of alchemy and the history of chemistry is one of the most extensive single collections of its kind, comprising more than 3000 separate monographs printed over a period of some 475 years. It is an excellent example of what may be accomplished by an ambitious collector within a period of only 10 or 15 years, when the field of interest is comparatively restricted.

Denis I. Duveen, born in London in 1910, was educated at Rugby School and Merton College, Oxford. Immediately preceding the last war he spent three years on research work, first at Battersea Polytechnic and afterward at the Collège de France, Paris, during which time he published a number of papers on optical activity of unsaturated secondary alcohols and the chemistry of naphthacenes. After a time as chemist in the Royal Gunpowder Factory he spent a major part of the war years as technical assistant in the Directorate of Ordinance Factories (Explosives) at the Ministry of Supply. He was afterward joint managing director of Ashe Laboratories, Ltd., London. He is at present engaged in chemical manufacture in the New York City area. Since the war he has published a number of papers on historical and bibliographical matters relating to the history of chemistry.[1]

With the recent purchase of his collection by the University of Wisconsin, this great wealth of source material has now become available to chemists, historians, and the public at large. The Duveen books now at Wisconsin comprehend, with a few exceptions, all of the volumes described in the printed catalogue.[2] At the time the library was offered for sale, Mr. Duveen indicated that he expected to retain all items by or about Lavoisier,[3] besides a few other books, at the same time adding to the collection quite a number of other volumes acquired by him since the printing of the catalogue.

Some idea of the scope of the library may be formed by the following statistics showing the present holdings grouped according to their century of imprint:

Incunabula	8
16th century	256
17th century	984
18th century	905
19th century	554
20th century	310
Total	3017

Since this count is exclusive of a few bound volumes of pamphlets and offprints not yet analyzed, the total number of separate items reaches well over 3000. It may therefore confidently be estimated that the total number of volumes amounts to considerably more than that of the great Young Collection at the Glasgow Institute of Technology. The latter library has been admirably catalogued by John Ferguson in his monumental "Bibliotheca Chemica" (Glasgow, 1906), long recognized as the classic work of reference on the subject. The Duveen catalogue, printed in an edition of only 200 copies, has now become a scarce item. It has obviously been modeled on Ferguson's work, though it lacks the scholarly references following the descriptions of many of the items.

As stated in the preface to the catalogue—and clearly evident from the statistics quoted above—the Duveen collection differs from that of Young notably in its emphasis upon works of the seventeenth to twentieth centuries, more than half the volumes being of the seventeenth to eighteenth centuries, whereas Young's library is richer in the earlier material. Thus the two collections are, in a way, complementary and their catalogues may conveniently be used to supplement each other as reference tools for any extended study of the historical development of chemistry.

Among the rarer items in the Duveen collection may be mentioned a first edition of Boyle's "Essays of the strange subtility of effluviums" (London, 1673) with the inscription: "For Mr. Isaac Newton from the Authour." This inscription is stated in the catalogue to be in the hand of Henry Oldenburg, secretary of the Royal Society, who was probably intrusted by Boyle with the distribution of copies of his work to certain select members of the Society.[4]

Another noteworthy Boyle item is his "An historical account of the degradation of gold," the rarest of all Boyle's works, here represented in a second edition (London, 1739). Of this, Boyle's only work devoted entirely to alchemy, Fulton knows only four copies, in all, of the first and second editions.[5]

[1] *Ambix*, **2**, 192–5 (1946), **3**, 26–32 (1948); *The Library* [5], **1**, 56–61 (1947); *Endeavor*, **7**, 116–21 (1948); *Chymia*, **2**, 111–7 (1949); *J. Hist. Medicine & Allied Sci.*, **5**, 448 (1950); *Isis*, **41**, 168–71, 281–3 (1950).

[2] "Bibliotheca alchemica et chemica. An annotated catalogue of printed books on alchemy, chemistry, and cognate subjects in the library of Denis I. Duveen," E. Weil, London, **1949**.

[3] The University Library, however, already holds a fairly complete list of the standard works by and about this chemist.

[4] *Cf.* FULTON, JOHN F., "A bibliography of the honourable Robert Boyle," *Oxford Bibliographical Society, Proceedings and papers*, **3**, 74 (1931–33).

[5] *Cf.* FULTON, J. F., *op. cit.*, pp. 89–90. A copy of the first edition (London, **1678**) in the Thordarson collection at Wisconsin is apparently in better condition than the only other recorded copy in the British Museum.

Besides the above two items, the Duveen collection contains 15 additional works of Boyle, all in first editions.

Another notable Newton item is the first English edition of "Secrets revealed" (London, 1669) by the mysterious "Eirenaeus Philaletha," said by some to have been the notorious English apothecary and adept, George Starkey. The volume is profusely annotated by Newton, having come originally from the collection of the Viscountess Lymington, Newton's grandniece.

A particularly rare and curious item is the "Amphitheatrum sapientieae" by the sixteenth-century German mystic, Heinrich Khunrath (1560?–1605), in a first edition, probably printed at Hamburg in 1595. Of this great work on theosophical alchemy and symbolism, consisting of a series of elaborate cabbalistic engravings, only one other copy is known, in Basle, Switzerland. The Duveen copy has the engravings hand colored and illuminated in gold and silver.[6]

Of a content similarly curious and rare is a series of 40 small Italian pamphlets containing "Books of Secrets," all of the sixteenth and seventeenth centuries. These little chapbooks, designed solely for popular consumption, and by so much the rarer to find surviving in any condition, cover subjects ranging from quack remedies, "household hints," and cooking recipes to astrology, palmistry, and legerdemain. Not one of them is mentioned by Ferguson in his classic work on the subject, "Bibliographical notes on histories of inventions and books of secrets" (Glasgow, 1883–1915).

The entire collection is rich in related works on various occult sciences, including Rosicrucianism, and notably represented by such figures as Brouault, Cagliostro, Isabella Cortese, Dee, Fioravanti, "Hermes Trismegistus," Haydon, Khunrath, Longinus, Thomasay, Ulstadt, Thomas Vaughan, Waite, and Welling. There are also copies of the "bibliographies" of F. Leigh Gardner and Caillet.

One of the rarest of the purely alchemical books is a small quarto volume containing an Italian translation of the "Rosarius Philosophorum" by Arnoldus de Villanova, probably printed at Venice shortly after the year 1500. Apparently the only other recorded copy of this book is now at the Walters Art Gallery, Baltimore, and has been described by Reichling, quoting from an Olschki catalogue.[7] The volume is also described in the catalogue of incunabula at the Walters Art Gallery,[8] but in both cases imperfectly, since the copy in question lacks the last two leaves, present in the Duveen copy. The latter may therefore rightly be described as the only recorded complete copy of the earliest known printing of an alchemical text in a vernacular language.

A more practical, though perhaps equally rare work, is a series of eight lithograph plates representing the interior of Justus Liebig's laboratory at Giessen, the first important scientific laboratory for purposes of instruction in chemistry. This work, printed at Heidelberg in 1842 in only a very few copies, was prepared by Liebig in response to requests from certain of his friends for a graphic explanation of his laboratory. Of especial interest and rarity is the plate portraying Liebig's pupils, several of whom became famous chemists in later years. This plate is missing from most extant copies.

A unique item of the greatest interest for modern chemical history is a copy of an English translation of Karl Wilhelm Scheele's "Chemical Essays" (London, 1786). This copy is profusely annotated with marginal corrigena and addenda in the hand of the eminent eighteenth-century Scotch chemist, Joseph Black.

All in all, the Duveen collection contains at least 490 works in first printed editions, and probably as many more in first translated editions. Further examination and study may reveal many more.

The value of the collection to the historian of chemistry lies primarily in its fine representation of works from the various periods, as stated above. The titles published in the sixteenth century deal with alchemy, medicine, and metallurgy, by far the greatest number being in the alchemical field. Seventeenth-century titles show a gain in the number of works of medical importance, while some of the alchemical works show a trend toward the clear recording of chemical fact as contrasted to the general mysticism evident in the earlier books. The eighteenth-century books show a continuation of this trend, alchemical books having become somewhat rare by this time. Many of these volumes deal with phlogiston, not only in the early part of the century when Stahl was writing voluminously, but also near the end when Lavoisier and his followers were attacking the theory, while the phlogistonists were fighting a vigorous but losing battle. At the same time, the fundamental works of the great chemists are well represented in this and the succeeding century. Titles published in the twentieth century are comparatively small in number and represent chiefly biographies and histories.

The collection includes a number of periodicals of great rarity, most important of which are the Crell journals. Between 1778 and 1804 Lorenz Crell published, under six different titles, the first journals devoted exclusively to pure and applied chemistry. Of the 75 volumes published, all but the last, *i. e.*, Volume 13, for 1786 of *Die Neuesten Entdeckungen in der Chemie*, are included in the collection. The Crell journals contain a great deal of the German thought during the period in which the phlogiston battle was being fought, and are also rich in other German investigations made during the last quarter of the eighteenth century. Even the famous Young collection is without most of these volumes, having only Volumes 2 to 13 of *Die Neuesten Entdeckungen* the last of the six journals.

[6] *Cf.* DUVEEN, D. L., *The Library* [5], **1**, 56 (1947).

[7] REICHLING, DIETRICH, "Appendices ad Hainii-Copingerii Repertorium bibliographicum. Additiones," Monachii, 1905–11, Fasc. II, p. 11, no. 405.

[8] "Incunabula typographica. A descriptive catalogue of the books printed in the fifteenth century 1460–1500 in the library of Henry Walters," Baltimore, 1906, p. 45.

Other periodicals in the collection include: *Annales de Chimie*, published by Lavoisier and his associates (18 vols., Paris, 1789–93); *The Chemist* (2 vols., London, 1824–25); *The Annals of Chemistry and Practical Pharmacy* (1 vol., London, 1842–43); *Chemical News* (1 vol., London, 1860); and *Journal of the Alchemical Society* (3 vols., London, 1913–15).

In somewhat the same category are society memoirs. The English edition of the essays of the Accademia del Cimento are included. The Accademia, the first organized scientific academy, flourished in Italy between 1657 and 1667. Also available are the three volumes of the *Mémoires de physique et de chimie de la Société D'Arcueil* (Paris, 1807–17). The Société will be remembered as the group which gathered about Berthollet in the later years of his life at his estate at Arcueil. Authors of papers in these volumes include Arago, Biot, Chaptal, de Candolle, Humboldt, Laplace, and Thenard. Gay-Lussac's paper on combining volumes was first published here. The Duveen copies, given to de Saussure by Berthollet, include Volume 3, containing pages 513–613 which are usually missing from other copies because the sheets were lost.

There are numerous library, auction, and dealers' catalogues, besides many standard reference works in the collection, including the bibliographies of Bolton, Ferguson, and Poggendorff, Sarton's "Introduction," Brunet's "Manuel," and the Joecher-Adelung "Gelehrtenlexicon." Not only are there included the older histories of Berthelot, Chevreul, Gmelin, Hoefer, Jagnaux, Kopp, Ladenburg, Schorlemmer, Thomas Thomson, and Wurtz, but also the more recent works of Armstrong, Hugo Bauer, J. C. Brown, Dobbing, Ekecrantz, Marsh, Stange, Stillman, and Thorpe. The historical works on alchemy by Read, Waite, and Muir are supplemented by those of Allendy, Eysenhardt, Ganzemüller, and O. S. Johnson. Related items of interest are Beckmann's "History of inventions, discoveries and originals" (London, 1846), Ellis's "History of fire and flame" (London, 1932), White's "History of the phlogiston theory" (London, 1932), and Kuhlmann's "Expériences pour servir à l'histoire de l'alcool, de l'esprit de bois et des éthers" (Lille, 1840).

The development of the modern textbook shows its beginnings in the copies of Libavius's "Alchemia." The collection has the very rare first edition of 1597 as well as the much enlarged second edition of 1606, both printed at Frankfort. Of Lemery's "Cours de Chymie" the collection contains the second French edition of 1677, the Spanish editions of 1703 and 1710, the second English edition of 1686 and that of 1720, the German edition of 1705, and French editions of 1713 and 1756. Boerhaave's "Elementa Chemiae" is present in the "surreptitious" Latin edition of 1724, the first and second Latin editions of 1732, the abridged English edition of the same year, the Dallowe English Translation of 1735, Peter Shaws' English Translation of 1741, and the second French edition of 1752. The texts of Macquer (1749–58), Marggraf (1767–68), Fourcroy (1782–1806), Nicholson (1790–96), Marcet (1809), Dalton (1808–27), Davy (1812), Thomas Thomson (1818–31), Daniell (1839–43), and Odling (1861) are also of interest. Texts on special branches of chemistry include Fresenius's "Instruction in chemical analysis" (London, 1846), Bunsen's "Gasometrische Methoden" (Braunschweig, 1877), Davy's "Elements of agricultural chemistry" (Edinburgh, 1813), and Wöhler's "Grundriss der organischen Chemie" (Leipzig, 1874–75).

Alchemy is represented by works attributed to Arnoldus de Villanova, Ashmole, Roger Bacon (5 titles), Basil Valentine (16 titles), Bernardus Trevisanus, Dee, Drebbel, Flamel, Fludd, Geber (12 titles), Helvetius, Kelly, Kunckel, Lull (21 titles), Maier (16 titles), Mynsicht, Palissy, Quercetanus or Du Chesne (11 titles), and Sendivogius (25 titles), besides a large number of lesser figures. There are also critical and biographical studies of Arnoldus, Ashmole, Bacon, Dee, Flamel, Fludd, Geber, Lull, Maier, and Palissy.

Early mining and metallurgy are represented by Agricola, Biringuccio (6 editions), and the anonymous Probierbuchlein" in editions of 1530 and 1574.

Brunschwig's book on distillation is present in the first Flemish (1517), and the third German (1532) editions.

In the field of medical chemistry there are works of Barchusen, Digby, Gesner (10 titles), Glauber (26 titles), Thurneysser zum Thurn, Withering, and Paracelsus, the last being represented by 21 titles. In addition there are 14 critical and biographical works on Paracelsus, including the psychological study of Carl Jung.

The rise of the phlogiston theory is seen in 15 works of Becher and eight of Stahl. The fundamental developments leading to the overthrow of the theory are revealed in the works of Rey, Boyle, Mayow, Hales, Black, Cavendish, Scheele, and Priestley. Final gasps of the dying theory are to be seen in the the works of D'Arcet, Baumé, Kirwan, Gren, the evolutionist Lamarck, and the geologist Hutton.

The atomic theory is seen as it develops in the works of Boyle, Dalton, Higgins, Berzelius, T. Thomson, and Prout. Electrochemistry is represented by Volta, Nicholson, Davy, and Faraday. Mineral analysis and the discovery of new elements are brought out by Bergman, Scheele, Vauquelin, Klaproth, and Berzelius. Organic chemistry rises through the works of Chevreul, Liebig, Dumas, Gerhardt, Laurent, Kekulé, Berthelot, and Pasteur. Food and biological chemistry are represented by the works of Hales, Ingenhousz, Accum, Chevreul, and Mulder.

It is apparent that the value of the Duveen collection to the historian of chemistry lies in the completeness and balance of its works on European chemistry prior to 1900. It brings together in one place not only the standard works of reference, but also a great many of the rare though significant works which have hitherto been available only in widely scattered libraries. This collection, plus the extensive Thordar-

son collection acquired by the University several years ago, and a systematic library policy of adding significant works in the history of science as they become available is making the University of Wisconsin a center of research in the historical development of science.

GEORG-MARIA SCHWAB

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GEORG-MARIA SCHWAB, eminent physical chemist, presents a fine example of a scientist who was not overcome by the force of adverse circumstances. With head held high, he has always kept his eyes steadily on his life's objective: to teach chemistry effectively and to contribute to its progress by first class writing and research.

Son of the well-known journalist Joseph Schwab, Georg-Maria was born at Berlin on February 3, 1899. After World War I he entered the University of Berlin and received the Ph.D. degree in 1923. His dissertation, prepared under the guidance of E. H. Riesenfeld in Nernst's famous Institut, described the first preparation of pure ozone, its solidification, and the determination of its physical constants. Dr Schwab served as assistant to Nernst's successor, Max Bodenstein,[1] until 1925. In Nernst's school he was trained in classical physical chemistry; under Bodenstein he became a kineticist. However, he was not a mere follower of Bodenstein but, from the start, worked on his own problems, such as the thermal decomposition of methane, reactions in cold discharges, and the dissolution velocity of gases. In 1925 he migrated to the University of Würzburg and habilitated as Privatdozent in 1927. There he studied the catalytic decomposition of ammonia, active hydrogen, dissolution of gases, and photochlorination.

In 1928 Wieland invited him to the University of Munich; he was promoted to extraordinary professor in 1933. The work on heterogeneous catalysis was broadened with the aid of a number of collaborators. Some of the fields which engaged him and his students were: the kinetics of heterogeneous catalysis, the nature of the heat of adsorption, the poisoning of catalysts, the distribution of active centers, the properties of atomic chlorine and bromine, the influence of organic radicals on parahydrogen. During this period came his invention of inorganic chromatography.

Numerous papers record his findings. Among his books are "Physikalisch-chemische Grundlagen der chemischen Technologie" (1928) and "Katalyse vom Standpunkt der chemischen Kinetik" (1931). The latter text has been translated (with additions) by H. S. Taylor and R. Spence, and is used widely in American universities. Professor Schwab edited the monumental international "Handbuch der Katalyse," of which 7 volumes have appeared.

In 1939 the increasing racial persecution forced this distinguished chemist to leave his fatherland. He emigrated to Greece where for 11 years he headed an industrial research laboratory. Fortunately he was allowed to continue his purely scientific work, though difficulties arose during the German occupation. This troubled period was nonetheless fruitful. In addition to incidental studies (adsorbing carbon, turn-over transitions, parahydrogen, chromatographic studies) his kinetic studies led him to a conclusive theory of the electronic mechanism of metal catalysis.

He was appointed Professor of Physical Chemistry at the Athens National University of Technical Science in 1950. The next year he was recalled to Munich to take over the chair of physical chemistry, a post previously occupied with distinction by Fajans and then by Clusius.[2] However, Professor Schwab, imbued with the importance of active scientific cooperation, continues to give courses twice each year at Athens.

[1] OESPER, R. E., J. CHEM. EDUC., **15**, 151 (1938); SCHWAB, G. M., *Wien. Chem. Ztg.*, **46**, 9 (1943).

[2] OESPER, R. E., J. CHEM. EDUC., **28**, 321 (1951).

LABORATORY QUIZZING IN THE FRESHMAN CHEMISTRY COURSE

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METHODS of laboratory procedure, especially in the first-year college curriculum, have been under constant discussion, and various deviations in methods have been used. Most manuals are so written as to have the student write the answers to various questions scattered throughout the experiment. These sheets are then detached and handed in to be graded, or other similar techniques are used. There are many objections to such laboratory reports, chief of which is that the weaker students can very easily resort to copying the answers of the better students. Thus many reports become virtually duplicates of each other, and it is difficult to evaluate the relative merits of the students. Furthermore, there is no way to prevent several students from getting together before the laboratory session, and writing out most of the answers. This completely defeats the purpose of laboratory work, which is to supplement and clarify some of the principles discussed in classwork. Finally the amount of time needed for the instructor to read such laboratory reports, if he is conscientious, is entirely out of proportion to their value.

A different approach to the solution to this problem has been going on at Wayne University over the past two years with very satisfactory results. In this system the laboratory report is dispensed with entirely except when quantitative data are involved, in which case only the data sheets, with calculations, are handed in by the student. In place of the laboratory report the students are given a short written quiz during the last 15 minutes of the three- or four-hour laboratory period each week. Three or four questions are asked, pertinent to the experiment of the day and of such a nature that they may be answered in a few sentences.

The manual used[1] does not especially stress the filling in of blanks by the student as he progresses, although places are reserved for students to record any points they may consider important for future reference. In place of this, a list of study questions is appended to each experiment which stresses the important points in the experiment and acts as a guide to the student's thinking, and as a nucleus for the quiz questions which they may expect later. Often the quiz questions are taken directly from this list.

Several advantages of this system have become evident:

(1) Most important of all, the students as a whole are more serious about their work, and do not approach it as another routine chore to be completed as soon as possible. Knowing they are being quizzed on details in the experiment of which they have no advance knowledge, they are anxious to understand everything they do and to consult with each other and with the instructor about points of which they are uncertain. This they are urged to do, the thought being that this is an extremely valuable way of assimilating chemical knowledge.

(2) Students are not likely to waste time or attempt to "sneak out" of laboratory early. If they finish early they often get together to discuss difficult points before the quiz begins. Experiments are planned, however, to be long enough to leave little waste time.

(3) Since the quizzes are given at the students' laboratory desks, one of the unexpected advantages which developed has been an improvement in the cleanliness of the desks at the end of the period. Tests are inconvenient to take on dirty or wet desks, as the students soon found. Consequently the laboratories are now left in much better condition than was formerly the case.

(4) Since the quizzes are short, they require only a minimum of time for the instructor to grade.

(5) If the students are at all conscientious, their laboratory quiz grades are apt to be good, generally above the average of their more formal tests. This is deliberately planned. It helps the morale of the weaker student, and keeps up interest and enthusiasm in laboratory work.

Some disadvantages are to be expected, chief of which is the tendency for some students to cut short their experimental work before they finish, to study for the quiz. This can be met by an ingenious choice of quiz questions which involve points of technique, materials used, etc.. thus making it almost mandatory for the student to complete the work before he can answer the questions. There is, also, the question of the proper supervision of the test, which is more difficult than in the usual classroom. These objections are not serious, however, with attentive instructors, and the results obtained considerably outweigh the difficulties involved.

Variations of this system, such as oral quizzing in place of written quizzing, or longer laboratory quizzes at less frequent intervals, are possible, but the method outlined above has proved most satisfactory.

[1] SECRIST, J. H., K. H. GAYER, AND RICHARD B. HAHN, "A Laboratory Manual of General Chemistry," Wm. C. Brown Co., Dubuque, Iowa, 1950.

CORK-BALL EXPERIMENTS ON CRYSTALLINE AND MOLECULAR STRUCTURE

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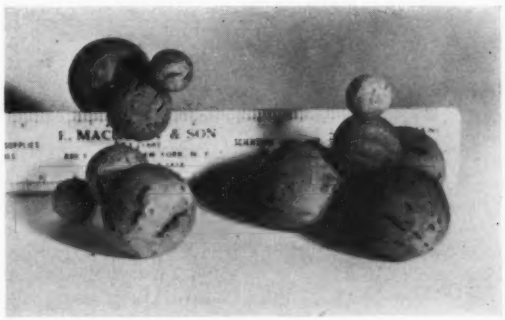
BECAUSE of the increasing emphasis on structural chemistry in freshman chemistry courses, the author believes that it is desirable to provide some laboratory experience with the subject at this level. It is not possible, in freshman laboratories, to study experimentally the electronic structures of the elements or any crystalline or molecular structures. However, the construction of models may be used to give the student a concrete three dimensional appreciation of the geometrical arrangements of atoms in various molecular and crystalline structures, and to stimulate him to calculate quantitative geometrical relations for some important structures.

In devising the experiments described here, it was desired to provide the student with materials with which he could assemble models realistically illustrating the packing of atoms in different structures, at least to the extent that the models should consist of spheres in contact, rather than open structures.

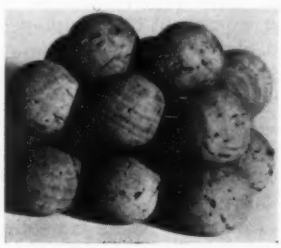
An economically feasible way of doing this is to use cork balls for atoms, and double-ended pins to join them. "X-quality" cork balls can be obtained from the Armstrong Cork Company at prices per 1000 balls of \$32.50 for the 1-in. size, \$20.00 for $^3/_4$-in. size, and \$9.25 for $^1/_2$-in. balls (other sizes are also available). The double-ended pins are made from ordinary household pins by cutting off the heads obliquely with a side-cutter. If this wedge-shaped end of the pin is inserted into a cork ball with pliers, a second cork ball may be pushed on to the sharpened end of the pin by hand without driving the pin deeper into the first ball. Each student is supplied with a pair of pliers and a wide-mouth bottle containing 20 one-inch balls and a cork "pin-cushion." For models requiring balls of more than one size, $^3/_4$-in. and $^1/_2$-in. balls are dispensed in test-tube containers.

In the first exercise, on the crystal structure of some of the elements, the students assemble a cube and calculate the values of the face diagonal and body diagonals in terms of the cube edge. These preliminary calculations assist them in understanding the structures studied subsequently. They next construct several units of a body-centered cubic structure, and calculate the relation between the cube edge and the atomic radius. They now derive the relation between the density, Avogadro's number, atomic weight, and the atomic radius (or the edge of the unit cube). A similar study is made of cubic closest packing; in addition, the relation between two dimensional closest packing in a hexagonal array and the two kinds of three dimensional closest packing is developed.



Trans $C_2H_2Cl_2$ and Chloroform, $^1/_2$-in. Balls for Hydrogen, $^3/_4$-in. Balls for Carbon, and 1-in. Balls for Chlorine

In an exercise on ionic crystals, the sodium chloride structure is constructed, and the type of coordination and the relation to cubic closest packing noted. The ionic radii of Na^+ and Cl^- are 0.96 and 1.81 Å; their ratio is 0.57, so $^1/_2$-in. and 1-in. balls represent the relative sizes of the two ions rather well. The $^3/_4$-in. and 1-in. balls are good for the construction of the cesium chloride structure, since the minimum radius ratio of cation to anion, to avoid anion-anion contact for coordination number eight, is 0.732.



A Body-Centered Cubic Structure

Several Hexagonal Layers of a Close-Packed Structure

Sodium Chloride, $^1/_2$- and 1-in. Balls

"Giraffic Chloride"

In the study of molecular structures, the relation of a cube to a tetrahedron is first noted. This makes it easy to calculate the tetrahedral bond angle and the ideal angle between a single and a double bond. Double bonds are represented by two bent pins. The geometrical structures of a variety of simple molecules are deduced in terms of the principles that the four electron pairs of a noble gas octet around an atom are at the corners of a regular tetrahedron, that a double bond involves sharing the electron pairs at two of these corners, and a triple bond requires sharing the electron pairs at three of the corners.

It is our experience that most of the students enjoy these laboratory exercises and that the exercises are essential and effective in getting the students to understand three dimensional relationships in structural chemistry. The idea that three-dimensional relations are readily comprehended and analyzed comes as a surprise to many students. This knowledge will presumably encourage them to think more frequently about the spatial geometrical aspects of problems they encounter in their field of specialization, whatever that may be.

The author will be glad to send a copy of the instructions for these experiments, as used at Caltech, to interested individuals.

ACKNOWLEDGMENT

The author acknowledges with pleasure that he first saw cork balls being used for the construction of molecular models in the laboratories of Professors Irving Klotz and Pierce Selwood at Northwestern University. The models were photographed by Mr. Richard Bachard.

A SPACE MODEL OF THE PERIODIC SYSTEM OF ELEMENTS

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Among the various schemes for representing the periodic system of the chemical elements as summarized by Quam and Quam (*1*), there are space models constructed by Crookes (*2*), Emerson (*3*), Soddy (*4*), Harkins and Hall (*5*), and Schaltenbrand (*6*). All are variations of the helix, but none gives an adequate position to the rare earths. In the models given by Payne (*7*). the position of the rare earths has not been improved. He states that the creation of a perfect periodic arrangement is probably impossible. Among the features that he specifies should be present are:

1. A position for hydrogen showing how it is related to the Li–F series.
2. A way of showing the relationship between the left and the right columns of a family.
3. A way of showing the relationship between the inert gases and the elements in the triads.
4. A reasonable position for the rare earths.
5. The position of the unfinished series.

In the space model here presented, some of the specifications may be considered to have been met.

A wire spiral is fastened to an upright iron support set in a circular hardwood base. Wooden beads strung on the wire represent atoms. On the planed side of each bead is a piece of cloth tape which bears the symbol of the element and its atomic number. On the base is a chart with radii numbered clockwise, beginning at the left: O, I, II, III, IV, V, VI, VII, VIII, I′, II′, III′, IV′, V′, VI′, VII′. Two short radii are inserted between VII-VIII and VIII-I′ to accomodate the first and third members of the triads. Two small circles have their diameters on radius O and are tangent to the center of the large circle. The curve on this radius is a projection of the wire to show the transition from the neutron to hydrogen to helium.

The system begins with the neutron on a vertical

extension of wire, placed so that its projection falls on radius O. The wire below it makes a reverse curve so that hydrogen, placed on its antinode, falls over radius I. Helium is at the next antinode, in line with the neutron and over radius O. Lithium follows above radius I, as the wire begins a clockwise spiral. Carbon is above radius IV, N over V′, O over VI′, F over VII′, and Ne over radius O. Another turn of the spiral places Na below Li, Si below C, etc., finishing with A over radius O.

At this point the spiral enlarges to the full circle of the base, so that elements of the long periods are above each radius. The inserted short radii allow places for the triads Fe, Co, Ni and Ru, Rh, Pd, etc. La comes over radius III, and the lanthanides, finishing with Lu, are placed on a loop of wire that stands vertically in the III-III′ plane. Hf begins another horizontal curve at radius IV, continuing to radius III. Here Ac starts another vertical loop for the actinides in the III-III′ plane, finishing with element 103. Symmetry indicates that another curve of the spiral would end with element 118 over radius O. Perhaps 118 cannot be formed but would disintegrate into neutrons, in which case the cycle would be complete from neutrons to neutrons.

Viewed vertically, all the inert elements are above radius O. All members of family I are above radius I or I′, family II is above radius II or II′, etc., the elements of the short periods being on small circles and the long periods on the large. The lanthanides and the actinides are placed so as to form a continuous part of the system, in a plane at right angles to the elements in the spiral.

Improvements can be made in the model, in addition to truing up the curves. Two parallel plane surfaces could be made on the beads so that the name of the element could be seen from more than one direction. The beads could be colored to bring out certain relationships. The scale adopted was merely one of convenience, large enough to be useful as a classroom teaching device, but not so large as to overburden the rigidity of the wire. Only three sizes of beads were used, although there might be a gradation in sizes to represent increase in atomic volume.

This model can be used to show students the essential relationships in the periodic system without bogging them down in detail. There are no makeshifts for accommodating the lanthanides and the actinides. Instead there is a continuity of sequence from the smallest to the largest atom, preceded by the neutron as the first condensation of primordial energy. The numbered beads following californium allow new names to be entered as additional transuranium elements are synthesized.

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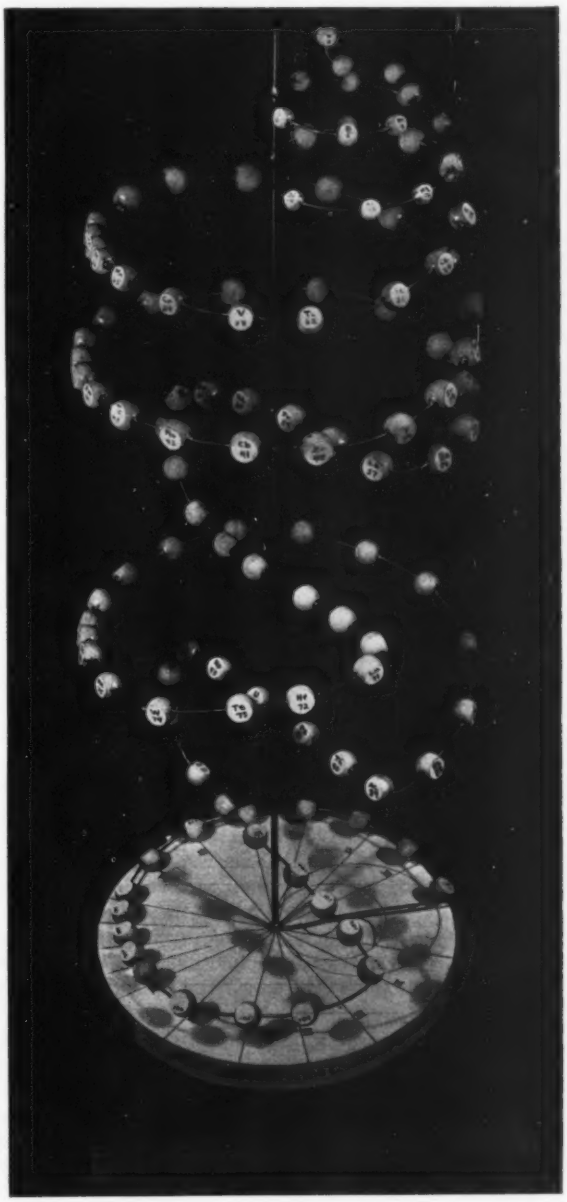

YOU MAY NEED A WITNESS

FLOYD TRIMBLE
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"WE THINK it is a matter of common knowledge that . . . research scientists are 'patent-wise'; if they are not they should be," said the court in Senkus *vs.* Johnston (*8*). The term "patent-wise" as so used, is broad, implying that the legal significance of such terms as interference, conception, reduction to practice, diligence, presumption, evidence, preponderance of evidence, burden of proof, and witness is understood. It, therefore, follows that no chemist can successfully plead ignorance of these terms in a patent interference case.

Under the law only one valid patent can be granted for one invention. When two or more parties at about the same time file patent applications covering the same invention, the Patent Office institutes a proceeding known as an interference to determine who in fact is the first inventor and entitled to the patent grant. The Patent Office regards an invention as a two-step process; the first being *conception* and the second, *reduction to practice*.

Conception as the term is employed by the Patent Office is a mental act meaning that the inventor has a complete mental grasp of the invention. The mental act of conception is rather easily proved by a written record in a notebook, by a memorandum, or by a communication to others. To constitute conception, however, the inventor's completed thought must be expressed in such clear terms as to enable those skilled in the art to make, compound, build the apparatus, or practice the process which is the subject matter of the invention. As the court stated in Field *vs.* Knowles (*6*), "Conception required as element of invention is the formation in mind of the inventor of a definite and permanent idea of the complete and operative invention as it is to be applied in practice—the mental process must have been completed and brought to a point where reduction to practice can begin, where invention stops and construction begins; it is not sufficient that the result to be obtained be conceived, but is required that there be conceived and disclosed the means provided to accomplish that result."

The second element of an invention, reduction to practice, may be either actual or constructive. "Actual" is that step in which the invention is tested under actual working conditions in such a way as to demonstrate its practical utility for its intended purpose beyond probability of failure. Actual reduction to practice occurs when a process is successfully performed, a machine is assembled, adjusted and used, a manufacture is completely manufactured, or a composition of matter is completely composed. The filing of a patent application adequately describing the invention constitutes a constructive reduction to practice and is legally equivalent to an actual reduction to practice.

In theory a determination of who is the first inventor (priority of invention) is simple; in practice it is often a difficult and costly procedure.

The broad rules employed for determining the first inventor are generally stated as follows: (1) The inventor with the first conception and the first reduction to practice is the first inventor unless he suppressed his invention until spurred into reactivity by the other party. (2) The inventor with the first conception, if diligent in reducing it to practice, is entitled to the patent as against one who conceived later even though the latter may be the first to reduce to practice.

The question of *diligence* is of no importance under the conditions of rule (1), but it is extremely important under the conditions of rule (2). What constitutes diligence depends to a large extent upon the particular facts of each case. To prove diligence an inventor must show that he made reasonable efforts to surmount any delay that he may have encountered in arriving at his actual reduction to practice.

In an interference the parties will be presumed to have made their inventions in the chronological order of the filing dates of their applications for patents involved in the interference. The first to file is known as the "senior party" and the others as "junior parties." In order to overcome the *presumption* that the senior party is the first inventor, the *burden of proof* as between applications is upon the junior party to establish the contrary by a *preponderance of evidence*.

Evidence broadly defined (*3*), is the means from which an inference may logically be drawn as to the existence of a fact; that which makes evident or plain. Evidence signifies that which demonstrates, makes clear, or ascertains the truth of the very fact or point in issue, either on the one side or on the other. Preponderance of evidence (*4*) has been defined as that superior weight of evidence which, while not enough wholly to free the mind from a reasonable doubt, is sufficient to incline a reasonable and impartial mind to one side of the issue rather than to the other. Where a presumption exists, more evidence is required to constitute a preponderance of evidence than where no presumption exists, and uncorroborated testimony of an interested witness will not overcome a presumption (*5*). Legally, presumption is a rule of law that courts and judges shall draw a particular inference from a particular fact or from a particular evidence, unless and until the truth of such inference is disproved. In

deciding an interference case this means that when a basic fact exists (the first to file) the existence of another fact (that he is the first inventor) must be assumed until the contrary is proved.

In interference cases the Patent Office and the United States Court of Customs and Patent Appeals follow the foregoing rules to the letter in reference to overcoming a presumption and the weight given the testimony of an interested witness. It has been said that these tribunals have gone far beyond what other tribunals require for corroboration in other controversial matters. One of the members of the Board of Appeals in the Patent Office in his dissenting opinion remarked that it was more difficult for an inventor to establish reduction to practice than it is to obtain a conviction for murder, Bambridge *vs.* Walton, (*2*).

To prove reduction to practice an inventor must show that he carried out the process or made the product or machine of the invention and demonstrated its utility. Inasmuch as he is an interested witness, it follows that he should present evidence in addition to his own testimony or his own records. Since the courts have held that all joint inventors are interested witnesses, the collective testimony of two or more joint inventors is no more satisfactory than is the testimony of a sole inventor. The application of the rule requiring corroboration is not based upon disbelief of the inventor. It is a rule of law which is applied under all circumstances—where the Patent Office and the court have no reason to disbelieve the inventor and even where they indicate that they believe him (*1*).

It might be suspected that the inventor's notebook entries together with his reports would be sufficient corroboration to satisfy the rule. A review of the cases shows that this is not so. In the case of Senkus *vs.* Johnston (*8*) the inventor in addition to his oral testimony, placed in evidence many entries from his personal notebooks, and reports and letters bearing upon the invention which he sent to others. The court held that although such documents were of value to establish conception of invention they were not sufficient to establish corroboration of reduction to practice for the reason that they were merely *self-serving* in their character. The term self-serving as so used covers declarations, either written or oral, made by a party in his own interest at some place and time out of court and they are not admissible as evidence in his favor.

Since the testimony of an inventor and his coinventors together with their records and reports is not sufficient, what can an inventor do to protect himself against loss of rights resulting from his inability to establish a reduction to practice? Apparently the only safe procedure is to have some one who is not a coinventor observe or carry out every operaton involved in the reduction to practice. This rule was adhered to by the court in Senkus *vs.* Johnston (*8*), affirming the Board of Appeals where the latter held that a corroborating witness of a process conducted by the inventor must testify that he observed on a specific date at least one specific experiment from start to finish with each process step conducted in consecutive order and with the ultimate preparation of the product which is the subject matter of the invention. Furthermore, in order that a corroborating witness be properly equipped to give testimony in a case such as this, he surely must see the operation of the process and have personal knowledge of the product. In addition to observing the invention, the witness must be one having sufficient training in the particular art to which the invention pertains to enable him to comprehend the invention fully.

Although the requirement of an independent coroborating witness to prove a reduction to practice seems harsh, it does not appear that the courts will relax the rule. In Israel *vs.* Cresswell (*7*), the court made this observation: "It is not infrequently complained that the enforcement of the rule works a hardship upon inventors. There are many laws which are recognized sound and necessary the enforcement of which, in some instances, works hardships. We can discern no good reason why the rule of corroboration in patent jurisprudence should do so, particularly since it is a rule of such long standing and one so universally known. In laboratories it should not have been a difficult task to reduce the invention to practice in the presence of competent chemists in the employ of the corporation who could have identified every requisite step that was taken." Similarly the court, in Senkus *vs.* Johnston (*8*), said: "We do not think that any material burden is imposed on any applicant for a patent to see to it that all of his work has been properly disclosed and explained to others who may be called as witnesses to corroborate the applicant's or inventor's evidence."

Although priority of invention has been proved by oral testimony alone, reliance upon such a method is dangerous. A better procedure is to maintain a written record giving the name of the witness, the extent of knowledge imparted to him, and the date. A fitting and proper place for this record is in the laboratory notebook.

The following is suggested as a method which meets the legal requirements of corroboration and in addition will provide a permanent record. The notebook may be any standard form, preferably one having a stiff cover with the pages sewn in as such a book will possess at least some evidentiary value. Each page should be numbered and dated. In order to refute any possible charge of later entries on the blank spaces, it is suggested that each page be completely filled before a new page is started. Either one side or both sides of each page may be used, but whichever method is used that plan should be followed consistently. Some people prefer to employ one side of the page for recording factual data and the other side for calculations. Such a plan is not objectionable if it is followed consistently. Since the notebook is essentially a record of facts, it is best not to burden the record with conclusions or opinions except those which are necessary

to explain the course of the experiment or to correlate data. Coined or trade names should not be used without identification. Avoid the use of abbreviations, with the possible exception of those approved by the American Chemical Society. Where mechanisms or devices are used, sketches may be used with explanatory matter as necessary. Pertinent references to auxiliary records such as notebooks of assistants, purchase orders, receipts, etc., which will serve to verify dates and show diligence should be entered. Errors should be crossed out, not erased, since erasures may give rise to a suspicion of subsequent alteration; for a similar reason entries should be made in ink or indelible pencil and if at all possible directly in the notebook.

Since there is no substitute for a corroborating witness, each experiment should be witnessed and a notation to that effect with the date entered in the notebook. This can be accomplished by obtaining the signature of the witness thus affirming that he has "witnessed and understood" the invention. The inventor should, of course, also sign. A single witness is normally sufficient, but if two are used it is more likely that one will be available when needed. Finally it should be noted that a witness is not a witness to the entries made in the notebook but a witness to the the details of the experiment and his recollection of those details must be original, not second hand; the notebook is a tool for refreshing his memory.

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THE STUDENT AFFILIATE CHAPTER AT THE TENNESSEE A & I STATE UNIVERSITY

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The Tennessee A & I State University, Nashville, Tennessee

The Tennessee A & I State University Student Affiliate Chapter, although new in the Department of Chemistry as an organization (granted a charter March, 1950), is the outgrowth of an active chemistry club which has been in continuous existence since 1945.

It was thought that an affiliate Chapter, meeting monthly, would be more effective than a chemistry club in (1) developing a professional attitude, (2) providing a link with a professional organization that would lead students to participate in the American Chemical Society as graduates, (3) keeping the chemistry students abreast of new developments in chemistry, (4) maintaining contacts on a national scope, (5) developing abilities in locating material and writing and speaking on chemical subjects, and (6) learning early to place value upon good public relations.

The initial membership consisted of 14 chemistry majors. Because of the desire on the part of non-chemistry majors to be associated with the program of the Affiliate Chapter, two types of members were accepted to membership of our chapter: members (chemistry and engineering majors) and associate members (students majoring in allied fields such as home economics and agriculture).

In effecting the purposes of this Chapter, annual programs are arranged by student members under the guidance of faculty sponsors. This annual program is completed in May for the succeeding school year. Each program is divided into a series of chemical activities in accord with the ability and interest of the members, and small projects which are closely tied in with the over-all departmental instructional program. Implementation of the planned activities and projects assures frequent student-teacher contacts and cooperation at several desirable points and professional levels.

A typical annual program developed by this chapter is shown by the following program and projects.

Program

1. Business and social meeting with freshmen: Prospectus of Department—opportunities, curricula, etc. Membership drive.

2. High-School Night. Glass blowing and student demonstrations.
3. Panel discussion: Peace and war uses of atomic energy.
4. Movie: "The Tale of Two Cities."
5. Demonstrations: Qualitative analysis students and general chemistry students.
6. Chemical tour to du Pont Chemical Plant, Old Hickory, Tennessee.
7. Business meeting
   a. Summary of year's work
   b. Election of officers for 1952–53
   c. Planning program for 1952–53

Projects

1. Publication of *The Test Tube*, official organ of the Student Affiliate Chapter (4 issues).
2. Operation of student loan library.
3. Keeping bulletin boards.
4. Announcements of Chapter meetings (electric sign replica of pin, written notices).
5. Exchange meeting with Student Affiliate Chapter at local university.
6. Assisting chemistry staff with freshman quarterly reviews (senior members).
7. Certificates for meritorious service to Affiliate Chapter.
8. Freshman Award.

Officers for 1951–52

John W. Thomas, *Chairman*
Robert Chess, *Vice-Chairman*
Charles Townsend, *Secretary-Treasurer*
Carl M. Hill, *Faculty Sponsor*
James U. Lowe, and
Mary E. Hill, *Co-Sponsors*

For each of the activities listed under "program" and "projects," the dates and persons responsible are included.

Classes (*i. e.*, freshmen, sophomores, etc.) often give demonstrations which create much interest owing to the rivalry involved.

Prizes given are based upon evidence of planning, setup of experiments, and ability to explain the principle or principles involved.

The first meeting of the year is usually a business and social meeting to which freshmen are invited. At this meeting a drive for membership is made; a short panel is held on the advantages and responsibilities of membership. Local membership cards, the insignia that may be worn, and the journal obtained (*Chemical and Engineering News*) are shown. In addition, curricula and opportunities of the department of chemistry are discussed.

During high-school night, Student Affiliates are host to junior and senior science students from local high schools. At this time "Open House" in the Department is held, followed by spectacular entertaining and informative demonstrations by affiliates and occasionally by faculty members who possess some particular skills, such as glass blowing. A social hour follows, during which time chemical souvenirs are distributed.

Outside speakers are sometimes presented. Usually these persons are from nearby institutions. For example, during the past two years the Affiliate Chapter at a neighboring institution and our chapter have held at least one combined meeting annually, at which time one of the professors is featured in a lecture on a topic mutually agreed upon.

Along with demonstrations, panels, and lectures, movies are shown on topics of interest. "The Tale of Two Cities," (the story of the bombing of Nagasaki and Hiroshima) was shown. The interest in this picture was high because one of the chemistry majors (a veteran) had gone into Nagasaki shortly after it was bombed, and could from first-hand information confirm the devastation and tragedy shown in the film.

Due to the presence of industrial plants nearby, one of the monthly meetings is devoted to a tour of some plant. The trip through the du Pont Chemical Plant (where rayon is manufactured) at Old Hickory, Tennessee, was especially worth while. Each step in the process of making rayon was shown in a well-planned and excellently guided tour. An abundance of literature was received.

At the final meeting, the year's work is summarized and evaluated, the election of officers takes place, and the program for the succeeding school year is planned and placed upon the departmental and school calendars. Thus everyone knows in advance what is to be done and who is to take responsibility for each meeting.

The Chapter is an invaluable asset to the chemistry major. A sustaining force has developed not present in the chemistry club. Through a community of interests and active participation a growing professional pride is developing, which we hope will grow and carry over into the chemical professions and chemical occupations.

The Test Tube, the official organ of the Student Affiliate Chapter, is published four times a year. (The name was chosen because in the minds of the students, it symbolized the spirit of chemistry.) Participation in the publication of *The Test Tube* has added to the interest and professional attitude of the chemistry students. It brings them into closer contact with each other, provides an opportunity for the university community to become acquainted with the accomplishments of the chapter, and gives expression to chemical interest through writing, reviewing, and drawing cartoons.

Another project which Student Affiliates have participated in is a loan library consisting of books, pamphlets, and magazines donated by the faculty and especially useful to students who are not majoring in chemistry but are taking one year of general chemistry.

In order to reward loyalty and service to the advancement of the Student Affiliate Chapter, certificates for meritorious service are given. These certificates may be received by members or associate members.

For the encouragement of scholarship among freshmen an award is presented on Honors Day to the freshman who earns the highest average in general chemistry obtained from grades, teacher recommendations. and a standard examination in chemistry.

Although the chapter is not old, and there are many improvements to be made, all programs except one have been held, and all projects carried out. We think that through the provision of strong and interested faculty sponsorship, we shall be able to fulfill our purposes

NATIONAL COOPERATIVE UNDERGRADUATE CHEMICAL RESEARCH PROGRAM

ETHALINE CORTELYOU
Argonne National Laboratory, Chicago, Illinois
W. P. CORTELYOU
Roosevelt College, Chicago, Illinois

THE National Cooperative Undergraduate Chemical Research Program was organized at the St. Louis Meeting of the American Chemical Society in 1949 to stimulate undergraduate research and to supply usable chemical data, but it has had two important by-products:

(1) Stimulation of research interest in teachers of small schools who have previously felt handicapped by limited funds, facilities, time, and research quality students.

(2) A supply of student research assistants for research-minded teachers whose own schools are too small to supply many chemistry majors.

The major premise of the program is that usable chemical information can be obtained from duplicating work done by two or more undergraduate students in different schools, independent of and unknown to each other.

The program is set up under a system of research projects that are simple and objective enough for undergraduate work. Each project is subdivided into research units. Each research unit is of such a nature that acceptable results and a report can be expected in 50 laboratory hours, the equivalent of one semester hour of college credit.

Dr. N. A. Lange of the Handbook Publishers, Inc., will award a Certificate of Acceptance to each student submitting an acceptable report to a Project Director. Suitably checked data may be selected for publication in the "Handbook of Chemistry." Data obtained on several units of a project may be published as a paper with full credit given to all participating students and schools.

The current projects are outlined as follows:

(*a*) Typical unit assignment
(*b*) Special equipment needed
(*c*) Special chemicals needed
(*d*) Minimum student prerequisite
(*e*) Name and address of Director

Analytical Project

1. *Sensitivity of Inorganic Qualitative Analysis Reagents*
   (*a*) Determine the number of milligrams (in solution) of each of a group of cations required to give a characteristic reaction with 1.0 ml. of each of several different reagent solutions under specified conditions.
   (*b*) 24 small reagent bottles (30–100 ml.) with droppers in the stoppers, or any convenient means of counting the drops of solutions used.
   (*c*) Salts to prepare the cation solutions. Sometimes special organic reagents.
   (*d*) Completion of a course in systematic qualitative analysis.
   (*e*) W. P. Cortelyou, Roosevelt College, Chicago 5, Illinois.

Inorganic Projects

2. *Solubilities of Inorganic Sulfamates in Water*
   (*a*) Determination of the solubility of barium sulfamate in water at 20°, 30°, 40°, and 50°; includes preparation and purification of the barium sulfamate.
   (*b*) Screw-capped bottles of approximately 125-ml. capacity; suitable shaking device for six or eight bottles in a thermostat.
   (*c*) Reagent grade sulfamic acid.
   (*d*) Good background in quantitative analysis.
   (*e*) Sister Agnes Ann, Immaculate Heart College, Los Angeles 27, California.

3. *Solubilities of Inorganic Fluoride Salts in Organic Solvents*
   (*a*) Determination of solubility of potassium fluoride in dioxane at several temperatures.
   (*b*) Constant temperature bath; containers not affected by compounds used, probably polyethylene bottles.
   (*c*) The inorganic fluoride and the organic solvent used.
   (*d*) Elementary quantitative analysis and elementary organic chemistry.
   (*e*) John H. Walkup, Centre College, Danville, Kentucky.

Organic Projects

4. *Preparation and Characterization of Diamides from Methylene Bis-(4 phenyl isocyanate) and Alkyl or Aryl Bromides through the Grignard Reaction*
   (*a*) Condensation of the RMgX compounds of three or four alkyl or aryl bromides with methylene bis-(4 phenyl isocyanate) and subsequent hydrolysis to the diamide, methylene bis-(*p*-acyl aniline). The purification of the diamides and determination of physical constants.
   (*b*) Two-necked, round-bottomed flasks.
   (*c*) Methylene bis-(4 phenyl isocyanate), alkyl or aryl bromides, metallic magnesium for Grignard reagents, dry ether.
   (*d*) Elementary organic chemistry.
   (*e*) Roy G. Bossert, Ohio Wesleyan University, Delaware, Ohio.

5. *Preparation and Properties of Acylacenaphthenes*
   (*a*) Preparation and properties of 5-(*p*-nitrobenzoyl)-acenaphthene.
   (*c*) Acenaphthene, *p*-nitrobenzoyl chloride, etc.
   (*d*) Organic chemistry.
   (*e*) Edith J. H. Chu, Immaculate Heart College, Los Angeles 27, California.

6. *Characterization of Amines as Salts of Oxalic Acid*
   (*a*) Preparation and crystallization of desired salts and determination of melting points of these derivatives.
   (*b*) Good standard thermometer.
   (*c*) None, except the amines to be determined.
   (*d*) Two semesters of organic chemistry and preferably a course in qualitative organic chemistry.

(e) H. Cohen, Roosevelt College, Chicago 5, Illinois.

7. *Preparation of Certain Schiff Bases and Their Reduction with Lithium Aluminum Hydride*
(a) Synthesis of octadecylbenzylideneamine and reduction to N-octadecylbenzylamine.
(b) Three-neck flask, mechanical stirrer, Claisen flask, and, for most preparations, a vacuum pump.
(c) Lithium aluminum hydride.
(d) Elementary organic chemistry.
(e) Bertin L. Emling, St. Vincent College, Latrobe, Pennsylvania.

Physical Projects

8. *Determination of Refractive Indexes of Binary Liquid Mixtures at Various Concentrations and Various Temperatures*
(a) Determination of refractive indexes of dioxane-ethylene glycol solutions of various concentrations at some given temperature.
(b) Good refractometer, constant temperature water bath, circulating pump.
(c) The liquids to be used.
(d) Elementary quantitative analysis and elementary organic chemistry.
(e) R. I. Rush, Centre College, Danville, Kentucky.

9. *Binary Azeotropes to Members of Hydrocarbon Homologous Series with Glycol Ethers*
(a) Study of the vapor-liquid equilibrium compositions as a function of boiling temperature for azeotropic agent with one or more pure hydrocarbon homologues.
(b) Efficient fractionation column, Abbé refractometer, manostat desirable but not necessary.
(c) Carefully purified liquids.
(d) Physical chemistry taken concurrently.
(e) William F. Kieffer, The College of Wooster, Wooster, Ohio.

10. *Determination of Ternary Azeotropes Containing Water and Amines and/or Alcohols*
(a) Investigation of toluene, methyl cellosolve, and ethyl alcohol system.
(c) Carefully purified liquids of the particular system studied.
(d) Physical chemistry taken concurrently.
(e) H. E. Weissler, Incarnate Word College, San Antonio, Texas.

Any teacher interested in registering an undergraduate chemistry student in one of the research projects should write to the director of that project.

The National Cooperative Undergraduate Chemical Research Program has been mailing an annual catalogue to about 150 colleges. This article replaces the 1952 catalogue. Any teacher interested in directing a new project should write to one of the authors of this article as soon as possible, since a project is listed under the direction of the first person to request it. Previous catalogues carried a list of "Projects in Search of Directors." Such a list can be obtained upon request. An outline of any project for the 1953 catalogue must be sent to one of the authors by January 15, 1953.

A CUP OF LEAD AMALGAM, POROUS TO MERCURY

ARCOT VISWANATHAN and M. G. SURYARAMAN
Madras Christian College, Tambaram, South India

MERCURY is known to dissolve in certain metals and to diffuse through them. J. Henry (*2*) described an experiment in which mercury diffused through a lead rod held in the form of a siphon, and flowed over. E. N. Horsford (*3*) studied the rate of ascent of mercury through lead rods. J. Nickles (*4*) found that zinc was more permeable than lead to mercury. E. W. Blank (*1*) devised an experiment reported in THIS JOURNAL, in which mercury under its own pressure passed down through a lead rod six inches long.

A cup of lead amalgam which is porous to mercury, but not to water, can be made as follows. Boil 20 ml. of mercury in an iron cup (85 ml.), taking care not to breathe the fumes which are poisonous. Using an iron file stir in 254 g. of lead (foil or tubing) and remove any floating scum. After cooling the amalgam to a semisolid state press a smaller iron cup (50 ml.) into the amalgam, holding it there until the mass sets. Then dislodge the cup of lead amalgam thus formed.

This cup is semiporous since it filters mercury but not water. In repeating the experiment, re-use the mercury which runs through the cup, since some lead dissolves in the process. The composition of the alloy can be varied somewhat without altering its curious property.

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(2) HENRY, J., *Proc. Am. Phil. Soc.*, **1**, 82 (1839).
(3) HORSFORD, E. N., *Am. J. Sci.*, [2] **13**, 305 (1855).
(4) NICKLES, J., *Compt. Rend.*, **36**, 154 (1853).

Proceedings of the

PACIFIC SOUTHWEST ASSOCIATION OF CHEMISTRY TEACHERS

NEW OFFICERS

THE new president of the Association is H. Murray Clark, Associate Professor of Chemistry at San Jose State College. He did his undergraduate work at Bradley University of Peoria, Illinois, and at the University of Illinois where he obtained a B.S. in 1936 and a Ph.D. in 1940. His research work was in physical chemistry on a problem involving a new type of mass spectrograph. He then worked for six years with the Keystone Steel Company of Peoria as chemist and fuel engineer. Since 1946 he has been employed at San Jose State College where he teaches courses in nuclear chemistry, colloid chemistry, and similar subjects. He is the immediate past chairman of the northern section of the Association.

H. Murray Clark

Gertrude Witherspoon Cavins, new secretary of the Association, is a Professor of Chemistry and Science Education at San Jose State College. She obtained her undergraduate training at San Jose State College and did graduate work at Stanford University. She has been continuously employed at San Jose State College since 1929, where her duties have been numerous and varied. At the present time, in addition to teaching analytical chemistry, cosmetics, and observation of secondary-school science teachers, she serves as secretary-treasurer to the Santa Clara Valley section of the American Chemical Society, and helped in its organization. She is also chairman of the affiliate group of the National Science Teachers Association. In addition to her duties at San Jose State College, she is registrar and secretary of West Coast Nature School, an outdoor field program supervised by the science departments of San Jose State College and Long Beach State College.

Gertrude W. Cavins

Ernest L. Bickerdike, the new treasurer of the Association, is an Associate Professor of Chemistry at the University of California, Santa Barbara College. He did both his undergraduate and graduate work at the University of Southern California, from which he received a Ph.D. in 1937. His field of research was the rare earths. He has been at Santa Barbara College since 1938, coming there after having taught three years at Taft Junior College. He teaches general, analytical, and advanced inorganic chemistry at Santa Barbara College. He is a past vice-president of the Southern California Section of the American Association of Physics Teachers.

ANNOUNCEMENT

The U. S. Civil Service Commission has announced an examination for chemists, physicists, biologists, and biochemists in the field of radioisotopes, to fill positions in Veterans Administration Hospitals and Centers throughout the country. Salaries range from $4,205 to $9,600 a year.

To qualify for this examination, applicants must meet a basic educational requirement and must have had from six months to three and a half years of professional experience in the physical or biological sciences, part of which must have been concerned with radioactive materials or with the instrumentation involved in the detection and measurement of radiation. No written test will be given.

Further information about the examination and application forms may be obtained from most post offices, from Civil Service regional offices, or from the U. S. Civil Service Commission, Washington 25, D. C. Applications will be accepted for an indefinite period.

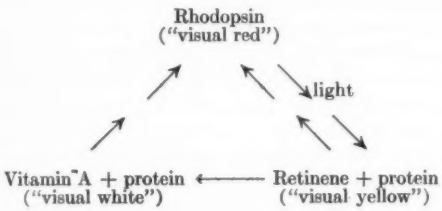
Report of the

NEW ENGLAND ASSOCIATION OF CHEMISTRY TEACHERS

THE MOLECULAR BASIS OF VISION[1]

GEORGE WALD
Harvard University, Cambridge, Massachusetts

In the rods of the retina, the red, light-sensitive pigment rhodopsin takes part in a cycle of the skeletal form (*1*):



Rhodopsin is a conjugated carotenoid-protein—a protein, opsin, joined with a carotenoid group to which it owes its color and sensitivity to light. The absorption spectrum of rhodopsin consists of three bands: the α- band, maximal at about 500 mμ, which is responsible for the sensitivity of rod vision in the visible spectrum; a small β-band in the near ultra-violet at about 350 mμ; and a tall γ-band at 278 mμ. The α- and β-bands go with the carotenoid prosthetic group, the γ-band with opsin. Light absorbed in the α- and β-bands bleaches the molecule; light absorbed in the opsin band seems to be unavailable for bleaching (*2*).

The bleaching of rhodopsin is a complex process, initiated by a light reaction, followed by ordinary chemical —*i. e.*, "dark"—changes. The light reaction can be isolated by irradiating rhodopsin at very low temperatures (−40° to −100°C.), or in dry gelatin films. Its product, *lumi-rhodopsin*, is only slightly different in spectrum—or color—from rhodopsin. At temperatures above −20°C., or at room temperature even in the dry condition, lumi-rhodopsin goes over in the dark to *meta-rhodopsin*, still with little change in color. Given access to water and at room temperature, meta-rhodopsin yields in the dark a mixture of regenerated rhodopsin and retinene plus opsin in roughly equal amounts (*3*).

[1] Abstract of a lecture delivered before the NEACT meeting with the Eastern Association of Physics Teachers and the New England Biological Association, at Brookline High School, December 8, 1951.

Vitamin A is the alcohol, $C_{19}H_{27}CH_2OH$. Retinene is vitamin A aldehyde, $C_{19}H_{27}CHO$. Retinene is reduced to vitamin A by an enzymatic reaction in which reduced cozymase (DPN-H_2) serves as coenzyme:

$$C_{19}H_{27}CHO + \text{DPN-}H_2 \underset{\text{retinene reductase}}{\rightleftharpoons} C_{19}H_{27}CH_2OH + \text{DPN}$$

The retinene reductase system can be assembled from its separate components in solution: the coenzyme, DPN-H_2; as substrate, synthetic retinene, prepared by the oxidation of crystalline vitamin A on manganese dioxide; and the apoenzyme, contained in a water extract of homogenized frog or cattle retinas. For the retinene reductase of retina, crystalline alcohol dehydrogenase from liver can be substituted. Both enzymes have very similar properties, and indeed, they may be identical (*4*).

This enzyme system introduces a second vitamin into the chemistry of rod vision, for the central component of cozymase is nicotinamide, the antipellagra factor of the vitamin B complex.

It has been known since Kühne that rhodopsin is synthesized in the retina in two ways: a relatively rapid "anagenesis" from yellow products of bleaching; and a much slower "neogenesis" from colorless precursors. Kühne believed "neogenesis" to occur only in the intact eye and to require the cooperation of the pigment epithelium. Correspondingly fast and slow modes of dark adaptation can be demonstrated in the human eye. "Anagenesis" can now be identified with the synthesis of rhodopsin from retinene, "neogenesis" with its synthesis from vitamin A.

Hecht and co-workers have confirmed Kühne's observation that solutions of rhodopsin, bleached in the light to retinene and opsin, on being replaced in the dark regenerate a small amount of rhodopsin—at most 15 per cent. We find that if such solutions are flooded with synthetic retinene, they regenerate as much as 85 per cent of their rhodopsin after bleaching. One can moreover extract from wholly bleached retinas colorless, carotenoid-free opsin and on mixing this with synthetic retinene obtain a large synthesis of new rhodopsin. This is a spontaneous, *i. e.*, an energy-yielding, re-

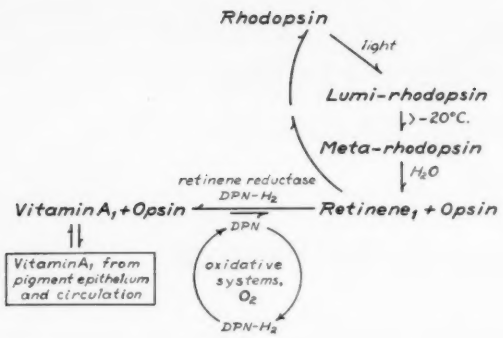


Figure 1

action. It is the *bleaching* of rhodopsin that requires energy, usually furnished by light (*5*).

Rhodopsin can also be synthesized *in vitro* from vitamin A. Isolated retinas, bleached in the light until the entire rhodopsin cycle has been reduced to vitamin A and protein, regenerate about 10 per cent of their rhodopsin in darkness. Homogenized retinas still perform this process. The addition of cozymase to a retinal homogenate approximately doubles the yield of rhodopsin. The further addition of pigment epithelium homogenate doubles the yield once again, bringing it to about 40 per cent. The pigment epithelium aids rhodopsin synthesis in a number of ways. One contribution is vitamin A itself. Indeed the simple addition of vitamin A to a retinal homogenate supplemented with DPN increases the yield of rhodopsin by about 60 per cent. The addition of oxidizing enzymes which keep cozymase oxidized also increases the yield of rhodopsin. All these means of supplementation of the retina used together—DPN, pigment epithelium, and respiratory enzymes—result in an over-all yield of rhodopsin about 60 per cent as great as it would have been during dark adaption in the intact animal (*6,7*).

What is the mechanism of this synthesis? The equilibrium between retinene and vitamin A, as shown above, lies toward the side of reduction, toward the production of vitamin A. In order to make the retinene reductase system oxidize vitamin A to retinene, it is necessary to introduce an aldehyde-trapping reagent, some substance which, by condensing spontaneously with retinene, removes it from the system as fast as it is formed. Such a substance is hydroxylamine, which condenses with retinene to form retinene oxime (*5*):

$$\underset{\text{retinene}}{C_{19}H_{27}CHO} + \underset{\text{hydroxylamine}}{H_2NOH} \longrightarrow \underset{\text{retinene oxime}}{C_{19}H_{27}CH{=}NOH} + H_2O$$

The retina, however, already contains a specific retinene-trapping reagent in opsin. As indicated above, opsin condenses spontaneously with retinene to form rhodopsin; and this exergonic process displaces the retinene reductase equilibrium in the oxidative direction, so driving the continuous endergonic oxidation of vitamin A to retinene. The latter reaction is also pushed from behind, so to speak, by the addition of vitamin A from the pigment epithelium, the blood circulation, and ultimately the diet; and by the reoxidation of DPN by the respiratory enzyme systems of the retina. The rhodopsin system therefore has the form shown in Figure 1 (*7,2*).

If this view of the functioning of the rhodopsin system is correct, it should be possible to reconstruct the entire system in solution by mixing four substances: vitamin A, cozymase, retinene reductase or alcohol dehydrogenase, and opsin. This is correct. Such a mixture, placed in the dark, forms rhodopsin; it bleaches in the light to vitamin A and opsin, and replaced in the dark it can form more rhodopsin (*7,2*).

This system was first assembled using a liver oil concentrate as the source of vitamin A. When some time later the same experiment was repeated using crystalline vitamin A, no rhodopsin was synthesized. It turned out that to make rhodopsin requires a specific stereoisomer of vitamin A. The ordinary crystalline vitamin A is *all-trans;* what is needed in this synthesis is one of the *cis* isomers. Actually the synthesis proceeds in two steps, the oxidation of vitamin A to retinene, and then the condensation of retinene with opsin to form rhodopsin. It is the latter reaction that is highly isomer-specific, and requires a *cis* isomer of retinene for it to go at all.

On the other hand the retinene which comes out of rhodopsin when it bleaches is the *all-trans* isomer. This must be isomerized, or replaced by the correct *cis* isomer, before rhodopsin can be regenerated. The continued functioning of the rhodopsin system therefore demands a continuous stereoisomerization of retinene or of vitamin A, or a continuous replacement of the *trans* forms of these substances by new supplies of the active *cis*-vitamin A from the blood (*8*) (see Figure 2).

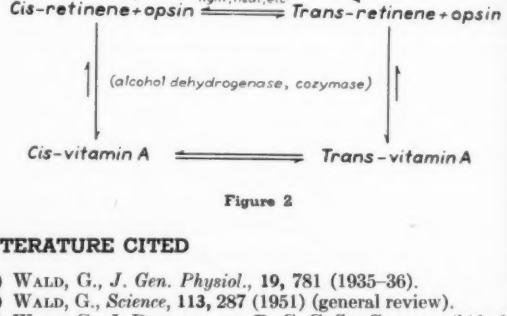


Figure 2

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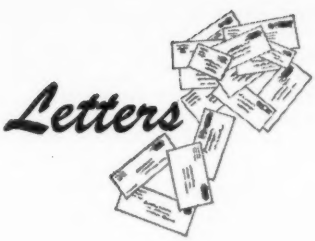

To the Editor:

The paper by Mr. Howard Nechamkin in the February issue titled "Laboratory Meetings Should Teach Too" impels me to write. What is said about the general chemistry laboratory scheme of things holds quite identically for the state of affairs in general physics, and it is about this which I wish to speak.

Throughout my teaching career I have very seriously doubted the usefulness and the efficacy and the pedagogical soundness of the general physics laboratory sessions. The usual scheme is familiar to all of us: three lectures per week (sometimes two, maybe four or even five), and laboratory meeting for two two-hour sessions or one three-hour session, or some comparable arrangement. We have done it *this way* for years but we have not too vigorously questioned the usefulness of it. Most of us now teaching it were taught it *this way* and those in our classes now will teach it *this way* when they acquire the cloak of erudition! Laboratory manuals have been written by the score. Indeed, few physics teachers exist who have not at one time or another written a manual. I wrote one too, for my own lab! And now what does the student do? This is thoroughly familiar to us. He comes to the lab, takes his place at a table whereon has been placed an array of equipment (sometimes much of it hooked up!), and proceeds, in sickening cookbook fashion, to hook this to that, read a scale or two, write down some data, and go home. The night before the report is due he "writes it up." This performance is a sheer chore, as can easily be verified (or recall your own days!), and what is learned by this whole affair is shamefully little. We are, it seems, committed to this scheme of things by tradition, by heritage, by inertia—but we are *dedicated*—which, I guess, is justification enough!

Now that I have narrated in this vitriolic, *negative* fashion, the query which logically follows is: what can we do about it? This has disturbed me no little and at the risk of utter heresy I have been "experimenting" with a scheme which gives promise. Interested persons can find the ideas put up in two brief notes in the March, 1951 *American Journal of Physics*, pages 190–91, bearing the titles "A departure in general physics laboratory procedure" and "What consitutes a laboratory examination." I have long felt that the scheme I here use in physics is adaptable to general chemistry, with some appropriate modifications which the chemistry men are well equipped to make. But new ideas are hard to sell!

We do not start laboratory until the tenth week of school. The first nine weeks are given entirely to lecture, theory, and problems, and a discussion of the experiments which might be pursued in the investigation of this theory. This provides the students with a sizeable backlog of physics, and precludes the very unsound practice of experiments coming far ahead of theory.

Ten or a dozen experimental setups are put up around the laboratory. Some lecturing is done on each, pointing up the measurements to be made, and the experimental difficulties.

Formal laboratory work is now taken up. Teams of two or three work together. *No manual is used.* The students are left to their own devices. The instructor advises here and there. Manuals are available but are seldom used. Their use is frowned on! No cookbook stuff! Data are taken in no prescribed fashion. The write up is brief, being sufficient for the student to crystallize his own thinking on the experiment and to state the results *discovered.*

Comments: The student brings some physics to the experiment. There is no cookbook performance. A spirit of *inquiry* and *discovery* prevails. Some now discover the *spirit of research!*

I would be happy to hear from chemistry teachers who have looked into the brief notes in the *American Journal of Physics.*

JULIUS SUMNER MILLER

DILLARD UNIVERSITY
NEW ORLEANS, LOUISIANA

To the Editor:

I should like to suggest that someone invent another term for the word resonance as applied to the field of molecular structure for the following reason.

The explanation of the stability of nuclei, not understandable in terms of classical mechanics, and the existence of compounds which are more stable than they should be in terms of classical theories of molecular structure are vexing problems. The concept of wave-mechanical resonance (W. Heisenberg) has been applied to these problems.

The explanation of the stability of nuclei involves the oscillation (resonance) of a particle between nucleons. In this case, a proton is a neutron part of the time and a neutron is a proton part of the time. According to the Yukawa theory, the oscillating particle is a negatively charged meson. On the other hand, the concept of resonance or resonating structures applied to molecules (L. Pauling) does not, of course, mean an alteration in the structure of the molecule with time. In this case, when resonance is said to occur between two structures, I and II, it does not mean that the structure of the molecule is I part of the time and II part of the time. Introduction of another term to emphasize this important distinction would simplify the situation for teachers and students of chemistry. The terms "mesomeric state" (C.

Ingold) and "benzene bond" (G. Wheland) are worthy of more attention and consideration.

FRANK BRESCIA

THE CITY COLLEGE
NEW YORK, NEW YORK

To the Editor:

The nation-wide furor over the "torch" sweaters is reminiscent of a similar situation whose abatement illustrated the effectiveness of methodical research.

During the Victorian era many of the poorer Irish wore night clothing made of Canton flannel. As the cottages and many of the houses were heated by open fireplaces, many serious accidents and even fatalities resulted because of the ready flammability of the fleecy material. Conditions became so bad that the authorities threatened to ban the sale of this goods unless the manufacturers found a way to flame-proof it. The problem was brought to the noted chemist W. H. Perkin (1838–1907). In due time he discovered a simple solution to the problem, namely treatment with stannic chloride.

When Perkin was asked how he happened to come upon this rather odd result, he smiled and said: "It was really very simple. You know that I have a large laboratory and it is well stocked with a large variety of chemicals, which are arranged on the shelves in alphabetical order. I began with 'A' and found what I was seeking in 'S.' "

If Perkin had been a German the story would be even more interesting; he would then have found it in "Z," since stannic chloride in German is Zinn Chlorid.

RALPH E. OESPER

UNIVERSITY OF CINCINNATI
CINCINNATI, OHIO

To the Editor:

The note by Professor H. B. Williams on "Elementary physical chemistry and mathematics" in the February issue deserves a commentary and some support.

I have experienced the same situation in physics classes, but the ideas here are not quite as illusive as those in physical chemistry. In thermodynamics the situation is truly appalling. The student often does not know whether he *can* integrate, or whether he *ought to*, or whether he *can't*. It is like pondering the verb in German: the reaction *may* take place; the reaction *should* take place; the reaction *must* take place; and so on.

I once fell heir to a class in physical chemistry (during the War) and the mathematical competence was frightful.

The answer lies here: mathematics teachers should constantly ask: "What does this *mean*?" If students saw the *physical meaning* of what they are doing mathematically in each step of the analysis the process would possess some sense. As it now stands the student can manipulate if it is set up for him in standard form, but if he has to *think* he is licked!

JULIUS SUMNER MILLER

DILLARD UNIVERSITY
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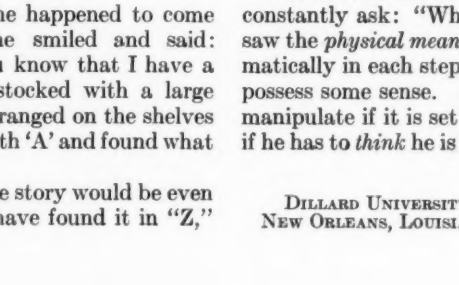

• A LABORATORY MANUAL OF QUALITATIVE ORGANIC ANALYSIS

***H. T. Openshaw,* Purdie Lecturer in Chemistry in the United College, University of St. Andrews. Second edition. Cambridge University Press, Cambridge and New York, 1951. viii + 95 pp. 32 tables. 14.5 × 22.5 cm. $1.75.**

ON FIRST glance the second edition of this little book appears to be identical with the first one published in 1946. For the new edition has the same format, paging, and charts of its predecessor. In fact, it was necessary to make a page by page comparison to see the changes. One finds on page 9 a new test for the presence of ester linkage is substituted for a quantitative saponification; two improved procedures are given on page 13; one hydrocarbon is added to the table on page 29; a completely revised table is given on page 50; preparation of methods for tertiary amines has been changed on page 73; and finally a new procedure for hydrolysis of anilides appears on page 74. A better quality of paper and finer binding are noted in this edition. Although not many changes have been made, it is the opinion of this reviewer that not many were needed.

All theoretical and class reactions are omitted in this brief book, as the author concentrates solely on analysis. The procedure suggested consists of four parts: (*a*) investigation of physical properties and physical separation of mixtures, (*b*) detection of constituent elements, (*c*) detection of functional groups and chemical separation of mixtures where applicable, and finally (*d*) preparation of derivatives.

In each case the material presented is surprisingly complete, and part (*c*) is excellent; actually a scheme of analysis is presented based upon what elements were found present in part (*b*). When the unknown has been identified the student is instructed to confirm his findings by means of a mixed melting point with the known. This suggestion has merit, though not all instructors may agree.

The major part of this book is devoted to methods of preparing

derivatives. Some of the general methods for nitration, bromination, and sulfonation, etc., are given; these are followed by page after page of specific directions for the preparation of derivatives of definite type compounds. Tables following each group list the boiling or melting point of the compound, and the melting point of at least three derivatives. Original references are given in most cases where specific derivatives are mentioned. The general references on page 20 will not prove to be too helpful in some cases.

This laboratory manual in itself may not be sufficient for a full semester or quarter course in qualitative organic analysis. However, in conjunction with one of the standard texts in the field it can be considered an excellent supplement. The information given is clear, precise, and can easily be followed by all students.

ARTHUR FURST

UNIVERSITY OF SAN FRANCISCO
AND CITY COLLEGE OF SAN FRANCISCO
SAN FRANCISCO, CALIFORNIA

• INORGANIC SEMIMICRO QUALITATIVE ANALYSIS

Carroll Wardlaw Griffin, Professor of Chemistry, Vassar College, and *Mary Alys Plunkett,* Assistant Professor of Chemistry, Vassar College. The Blakiston Co., New York, 1951. x + 299 pp. 14 figs. 31 tables. 15.5 × 23.5 cm. $4.75.

THE authors' aim, as stated in the preface, is twofold: "to teach the underlying principles of analysis" and "to develop in the student a method of execution . . . . based upon precepts which are clearly understood and techniques which are cleanly performed." To further these aims, the book is divided into two parts. Part I consisting of eight chapters (170 pages), includes atomic and molecular structure, chemical equilibrium, and oxidation-reduction theory. At the end of each chapter is an excellent set of problems (and answers) which will help the student to understand and use the material in the text.

Part II (82 pages), containing the laboratory instructions, introduces the student to the techniques of semimicro methods. The procedure is to study a few characteristic properties of some 25 cations and 19 anions followed by carrying out reactions used in analysis and finally in doing the group analysis. Diagrams and a discussion of laboratory techniques including methods of dissolving an unknown solid are given.

The various concepts introduced review Bohr's ellipitcal atom, elementary Werner theory, the Debye-Hückel theory of electrolytes and the Brønsted theory of acids and bases. Equilibrium is derived from kinetic considerations and includes problems related to the formation of precipitates, weak acids and bases, buffers, indicators, hydrolysis, complex formation, and oxidation-reduction reactions. Particularly important are sections dealing with problems of analysis; post-precipitation, occlusion, partial and selective precipitation, and colloid formation. Oxidation-reduction reactions are balanced by the useful ion-electron method.

It is unfortunate that the authors chose to relate electrode potentials to reduction rather oxidation reactions. A more serious criticism is the apparent lack in understanding of the nature of metallic coordination entities in particular and of inorganic chemistry in general. This is important as the majority of materials encountered in analytical chemistry fall into this category. On page 125, for example, the authors say that the coordination number "usually is twice the primary valence of the simple ion." On the preceding page, however, a copper complex having the formula $Cu(CN)_3^{--}$ is mentioned. Not only is the discrepancy not discussed, but the ion does not exist. In applying the rule to hydrated aluminum the formula $Al(H_2O)_6^{+++}$ is obtained (page 136). Here the authors ignore the fact that it is not possible to pack six water molecules around the aluminum ion.

The authors point out that if hydration is always understood it can be neglected. However, when such a practice leads to writing AlO_2^-, ZnO_2^{--}, etc., which have no structural meaning and which lead to inconsistencies, it should be discountined. Thus, for example, on pages 134 and 203, arsenite is written as AsO_2^-, while on pages 211 and 212, AsO_3^{---} is used. Again, whereas hydration and amphoterism appear to be somewhat related to complex formation (pages 134–8), the identical problem of the hydrolysis of metallic ions (pages 95–6) appears to be completely unrelated to either complex formation or hydration. Another inconsistency involving hydration is the proper use of NH_3, but the improper use of H_2CO_3.

More familiarity with the reactions themselves is shown in the experimental part of the book. Particularly good is the simplification in the handling of the second group by the oxidation of stannous ion by hydrogen peroxide and the reduction of pentavalent arsenic by iodide ion. Other salient features include the use of zirconium for removing phosphate ion, the use of the thiocyanate test for cobalt as an alternate to the awkward and not very good cobalti-nitrite test, the formation of a lake with *p*-nitrobenzene azoresorcinol in the confirmatory test for magnesium, and the formation of antimony oxysulfide in the confirmatory test for antimony.

Ten tables are placed in the appendix. These include reagents needed, properties of compounds, and various equilibrium constants.

The book will meet the requirements of most courses designed to teach the student the principles of qualitative analysis and the techniques and chemistry most often employed in problems involving inorganic chemistry.

RICHARD G. YALMAN

ANTIOCH COLLEGE
YELLOW SPRINGS, OHIO

• BRITISH SCIENTISTS

E. J. Holmyard. The Philosophical Library, Inc., New York, 1951. viii + 88 pp. Illustrated. 12.5 × 19 cm. $2.75.

"THE fame of British seamen, British colonizers, British inventors, and British sportsmen has long been worldwide. The same is true of British commmercial enterprise, British phlegm, and of the British genius for constitutional government. It is therefore a little surprising to observe the comparative lack of recognition accorded to the achievements of British scientists." With these sentiments as the introductory remarks to the first chapter of this little book one would expect to find mention of credit and praise that should be accorded to certain great British scientists who have been passed over almost in silence by continental writers. But the reader with such expectations will be disappointed, for with very few exceptions the 25 chapters give short accounts of the men generally recognized throughout the world as great contributors to the development of science.

It seems to this reviewer that the book is organized to show the portraits of 23 great men. Most of the portraits are those with which we are familiar—copies of portraits in possession of the Royal Society or the British Museum. The exceptions are those of Priestley and Faraday. Priestley must have been rather nice looking in the period of his work on electricity when he became a Fellow of the Royal Society. One wishes that instead of the pictures of Darwin, Lord Kelvin, Sir William Perkin, Sir J. J. Thomson, and Lord Rutherford which show them as they looked in their later years, there had been substituted pictures of them as they appeared in the period of their greatest intellectual activity.

The first chapters indicate Dr. Holmyard's knowledge and appreciation of early science in Britain, and the transitions from chapter to chapter are excellent. There are some serious omissions among the later figures, among them Robert Hooke and Thomas Graham. Graham's fundamental work, it is true, has received due recognition outside of Britain, but the same cannot be said for the contributions of Robert Hooke, certainly one of the most ingenious and prolific inventors Britain has ever produced and certainly also one of the greatest geniuses of that century of geniuses, the seventeenth century. There is no por-

trait of Hooke, but this does not wholly account for the omission for there is none of Robert of Chester, the great translator of the twelfth century, nor one of Bartholomew who wrote the most popular science textbook of the thirteenth century, "On the Properties of Things."

The accounts are very short, usually not more than two or three pages, but the selection of material is discriminating. There are a few errors: page 22, Boyle did not construct the air pump, this is the work of Hooke; page 24, Newton did not originate the idea of the calculus, this is most probably the contribution of another Englishman, Issac Barrow; page 74, cathode rays were discovered by Pluecker, not by Hittorf. There is no index. The Select Biographical Bibliography contains 23 titles, only a few of which will be new to the reader in the literature of science.

This is a relatively long review of a very small book almost exorbitant in price. Its appeal will be limited. The accounts are too brief to be of any real value to those interested in British contributions to the development of the fundamental work on which modern scientific achievements are based. But they may serve to whet the appetite of the general reader to pursue the subject further and in this event the book will have accomplished its purpose.

CLARA DE MILT

NEWCOMB COLLEGE, TULANE UNIVERSITY
NEW ORLEANS, LOUISIANA

● FUNDAMENTALS OF ATOMIC PHYSICS

Saul Dushman, **Research Consultant, Research Laboratory, General Electric Co., Schenectady, N. Y. McGraw-Hill Book Co., Inc., New York, 1951. x + 294 pp. 66 figs. 16 × 24 cm. $5.50.**

ACCORDING to the preface, this book owes its origin to the Fellowships granted annually by the General Electric Company to high-school teachers of science for the purpose of "enlarging each Fellow's grasp of new developments in the physical sciences." To achieve this end, six weeks' courses in physics and chemistry were given during the summer, and Dr. Dushman states that he cooperated in the preparation of a series of notes which might be of assistance in connection with the lectures in physics. "These notes were gradually developed into a more systematic survey of the fundamentals of atomic physics, which is the subject of this volume. It was thought that this review might prove useful not only to teachers of science. . ., but also to students of engineering."

The book opens with a brief history of physics, from 2000 B.C. to the present day, after which there is a short mathematical introduction in which the reader is taken from the binomial theorem to the solution of simple differential equations. Then follow chapters dealing with the kinetic theory of gases, properties and behavior of electrons, X-rays, atomic spectra, electronic configuration of the elements, and matter waves. In the presentation of this material the author has drawn frequently upon his previously published works.

The remainder of the volume, consisting of some 86 pages, is devoted to the nuclear aspects of atomic physics, such as radioactivity, isotopes, nuclear reactions, fission, and nuclear forces—topics which are of interest to the chemist as well as to the physicist. However, almost exactly half of this consists of a chapter on Generators for Acceleration of High-Energy Particles. Thus, other important subjects, such as nuclear fission, the separation of isotopes, nuclear binding energies, and the release of atomic energy, receive very brief treatment.

In his book, Dr. Dushman frequently quotes from other authors. These quotations are printed in small type, giving the erroneous impression that they are less important than the main text. Incidentally, the present reviewer feels that while quotations from primary contributors to the field of atomic physics add to the interest of a book, direct quotations from textbooks and other secondary authorities, which abound in the book under review, have merit only in special circumstances.

As has been implied in the foregoing remarks, this book will appeal more to teachers of physics, for whom it was intended, than to those who wish to learn how the newer developments in atomic physics should influence their teaching of chemistry.

SAMUEL GLASSTONE

LOS ALAMOS
NEW MEXICO

● THE PHYSICAL SCIENCES

Emmett James Cable, **Iowa State Teachers College;** ***Robert Ward Getchell,*** **Iowa State Teachers College;** ***William Henry Kadesch,*** **Adams State College of Colorado; chapters on astronomy by** ***Harry E. Crull,*** **Butler University. Prentice-Hall, Inc., New York, 1951. xvii + 496 pp. 345 figs. 22 tables. 18 × 25 cm. $5.50.**

THIS is the third edition of a popular, well-written physical science text. The book is written particularly for two types of students; the one preparing for teaching as a career and the one who needs a science background as part of a general education. The volume is well designed for its purpose.

The new size, type, and form will appeal to its readers. There is a large number of illustrations and tables, some of them new to this edition. The authors have managed to maintain a good balance between the encyclopedic approach to physical-science textbook writing and the presentation of concepts without fact and figures.

In the words of the authors: "The text has been revised at many points in the interest of greater clarity, or for the purpose of including more recent developments. Typical examples of the first kind of revision are found in the treatment of Brownian movement, the telephone, ionization, chemical equilibrium, and central forces. Examples of the latter kind are modern practices of the Weather Bureau in the collection of atmospheric data and in forecasting, silicones, atomic energy, and the means of studying atomic structure."

Probably the greatest change lies in the Astronomy section. The number of chapters had been increased from five to eight. The chapters are well written from an obviously broad background. For the moment they are up to date giving Kuiper's data on Pluto, a second moon for Neptune, and a fifth one for Uranus.

CLAUDE FELL MERZBACHER

SAN DIEGO STATE COLLEGE
SAN DIEGO, CALIFORNIA

● METALLURGICAL THERMOCHEMISTRY

O. Kubaschewski **and** ***E. Ll. Evans,*** **National Physical Laboratory, England. Academic Press, Inc., New York, 1951. xi + 368 pp. 103 figs. 14 × 22.5 cm. $6.**

VERY appropriately, this book is Volume I in a new series of monographs on Metal Physics and Physical Metallurgy. It assumes some previous contact with chemical thermodynamics and is aimed primarily to make the methods and data more readily available to process and physical metallurgists. The notation, given in a three-page introduction, is in general in accord with that adopted by the British Standards Institute in 1938. Exothermic reactions are regarded as those in which ΔH is negative. To avoid misleading inferences, a study of the notation system is a necessary prerequisite to perusal of the book.

In the first section the theoretical background is reviewed, using as a typical example the preparation of a pure metal by the reduction of one of its compounds. Solutions are illustrated by mixtures of metals and metal-slag systems. The next hundred pages are devoted to a discussion of experimental methods used to determine thermodynamic quantities in systems of interest to metallurgists. An important contribution is the detailed discussion of methods to estimate values for thermochemical vari-

ables when experimental data are not available, or to judge the probable reliability of published data. Comprehensive tables of thermochemical data fill the rest of the monograph, except for the final ten pages, which are devoted to application of thermochemistry to the following metallurgical processes: reduction of MgO with Si; reduction of $MgCl_2$ with H_2; deoxidation of steel with Si and with Al; and oxidation of alloys of Pt and of Au. The final example is the determination of boundaries in phase diagrams from thermochemical data.

In all, over 300 bibliographic references are cited. The literature had been reviewed through 1950.

The authors have done a creditable job of providing an up-to-date and practical aid for metallurgists and chemists. Judging by this informative book, thermodynamics is at long last really becoming a tool for the plant metallurgist. If during his schooling he was thoroughly prepared in classical thermodynamics, he will be able to accept this extension of thermochemistry to practical situations with ease. The result should be improved processes and increased production.

LAURENCE S. FOSTER

BELMONT
MASSACHUSETTS

• PAINT AND VARNISH PRODUCTION MANUAL

Edited by *Verne C. Bidlock,* President, Pennsylvania Color and Chemical Company, and *Edgar W. Farig,* General Superintendent, Lowe Brothers Company. John Wiley & Sons, Inc., New York, 1951. ix + 288 pp. 51 figs. 3 plates. 15.5 × 24 cm. $6.50.

THIS is a "practical" book in that it "has been designed for the education of production personnel in the manufacture of surface coatings and for those interested in the problems of management." The chapter headings best characterize the material found in the book: physical facilities; paint plant; varnish plant; products; engineering; manufacturing operations; personnel; factory cost accounting; research, development and control; fire protection, safety and health.

The chemical engineering instructor will find material in this book of interest in plant design courses. The chemistry instructor will find nothing on the chemistry of paint and varnish.

If you want to go into the paint or varnish business, secure a copy of this book.

KENNETH A. KOBE

UNIVERSITY OF TEXAS
AUSTIN, TEXAS

• DANA'S SYSTEM OF MINERALOGY. VOLUME II

Charles Palache, Emeritus Professor of Mineralogy, _Harry Berman,_ late Associate Professor of Mineralogy, and _Clifford Frondel,_ Associate Professor of Mineralogy, all of Harvard University. Seventh edition. John Wiley & Sons, Inc., New York, 1951. xi + 1124 pp. Illustrated. 15.5 × 23.5 cm. $15.

THE Dana books on mineralogy, by Yale Professors James Dwight Dana, Edward Salisbury Dana, and their associates, have long dominated American mineralogy at various levels, from the beginner's level to the advanced level of the "System." Dana's "System of Mineralogy," which was sometimes referred to as the "mineralogist's bible," was last revised in 1892 and supplements were added in 1899, 1909, and 1915. Early efforts to prepare a seventh edition convinced the authors that a thorough rewriting of titanic proportions was necessary. Harvard professors were appealed to for help and eventually the authorship passed to Professors Charles Palache, Harry Berman, and Clifford Frondel. Volume I appeared in 1944 under their authorship. Professor Palache has retired, Professor Berman was killed during World War II, and Professor Frondel has ably assumed the whole load of responsibility. Volume III is now in preparation.

The primary purpose of the "System" is to present descriptive data on all known minerals. Such general discussions as are included are restricted to a 48-page section of Volume I. The first volume contains descriptions of elements, sulfides, selenides, tellurides, sulfo salts, and oxides, excluding silica. The present Volume II includes descriptions of halides, nitrates, iodates, borates, carbonates, sulfates, chromates, phosphates, arsenates, vanadates, antimonates, molybdates, tungstates, organic compounds, etc. Remaining for Volume III are further organic compounds, but chiefly silica and the silicates. It would be a rare situation that would require one volume without the others.

The descriptive plan set in Volume I is followed in Volume II and includes such items as synonymy; crystallographic data, such as axial ratio, forms, crystal drawings, habit, twinning, goniometric measurements; X-ray data, such as space-group (Hermann-Mauguin notation), dimensions of unit cell, atomic content of unit cell; etch figures; physical properties, including cleavage, hardness, specific gravity; optical properties; chemical properties, including composition, isomorphous substitution, selected analyses, a few chemical tests for identification; occurrence; alteration; name; and references, with some comments.

This is no mere compilation from published literature, but includes much original work done to fill in some of the gaps and to bring the information up to date. Altogether, this is a work of utmost importance to mineralogists and should be of great value to chemists who work with mineral products or who have reason to refer to minerals or their chemical equivalents. The authors are to be congratulated on a magnificent accomplishment in bringing to completion these two, of three projected volumes. The publishers have contributed much to mineralogy by promoting the work of this seventh edition.

ALONZO W. QUINN

BROWN UNIVERSITY
PROVIDENCE, RHODE ISLAND

• HISTORY OF THE THEORIES OF THE AETHER AND ELECTRICITY

Sir Edmund Whittaker, honorary fellow of Trinity College, Cambridge. Philosophical Library, Inc., New York, 1951. xiv + 434 pp. 16 × 24.5 cm. $12.

THE first edition of this book, published in 1910, has been the authoritative reference work for the historical aspects of the theories of optics, electromagnetism, and the ether. This second edition will certainly continue to occupy the same position for many years to come. It is very welcome, for the book has long been out of print, and difficult to obtain. While this second edition contains material not in the first it brings only the story of the centuries preceding our own; another volume is promised, that will recount the achievements (and failures) of the twentieth century. The major addition to the table of contents of the present volume is a chapter on classical radiation theory. In this, the history of spectra and thermal radiation is given its proper place in the historical perspective.

The plan of the book is ambitious, and has been carried out with unusual success. Beside giving biographical material concerning the investigators in this field, their major contributions are explained in detail, analyzed, and related to those of others. This frequently requires mathematical derivations, which are presented with all the clarity and elegance for which Professor Whittaker has become famous. It is a true history of ideas, each of which is concisely and critically presented, but it might also serve as an introduction to the technical aspects of the subject. The scholarly effort that its author has expended cannot be adequately measured by the physical size of the volume; it is further concealed by his literary skill, which has produced a readable story rather than a dull compendium.

It has been and will continue to be a most influential book: this makes it necessary to point to a defect that might be politely ignored in a work of lesser magnitude. The present edition still presents the ether theory from a nineteenth century viewpoint.

This can be established by a glance at the index, which does not contain the names of Euclid and Lobatchewsky. Throughout the book, it is tacitly assumed that geometry is a branch of mathematics, not of physics. In discussing Descartes' reasoning, which established the logical necessity for postulating an all-pervasive medium that participates causally in the affairs of the universe, no mention is made of his implicit faith in the Euclidean doctrine of the passive immutability of space. It was this unreasoning faith that led Descartes to postulate the existence of an ether, rather than to ascribe the necessary physical properties to space itself. This same faith was shared by most of the men whose ideas are discussed in this book; but no mention is made of the manner in which it conditioned all their thought, and led to their unquestioning acceptance of the Cartesian duality of space and ether.

It must immediately be admitted that, had these matters been discussed with the requisite thoroughness, the plan of the book would have expanded beyond even Professor Whittaker's ability to accomplish it within the allotted pages. One hopes, however, that he will include the geometers of the nineteenth century in his second volume, and adequately assess their contribution to physical theory. For the relativity theory, which is loosely said to "deny the existence of an ether," cannot be comprehended historically except as the confluence of two streams of ideas both of which can be traced through the period considered in the present volume. The course of the one stream is beautifully described, but the reader is given no slightest hint of the other that flows in a converging course, hidden only by hills that were the horizon for nineteenth century physicists.

CARL ECKART

Scripps Institution of Oceanography
La Jolla, California

● MICROBIAL DECOMPOSITION OF CELLULOSE

R. G. H. Siu, **Research Director, Pioneering Research Laboratories, U. S. Army Quartermaster Corps, Philadelphia; Research Associate, Harvard University. Reinhold Publishing Corp., New York, 1951. xi × 531 pp. 128 figs. 150 tables. 16 × 23.5 cm. $10.**

Increasing specialization may be taken as one mark of the maturation of a science; "Microbial Decomposition of Cellulose" is an example of such development in microbiology. Whether or not Dr. Siu's interpretations receive wide acceptance, he is entitled to much commendation for his fine digest of an enormous but scattered literature and for the help his monograph will give to chemists, microbiologists, and the processors of cellulosic materials alike.

The book is centered on the microbial attack of cotton fabrics because of the position of the cotton fiber as the purest natural form of cellulose, because of postwar intensification of interest in the mildew-proofing of cotton textiles, and because the author had access to much obscure information on cotton deterioration. Basing his work on some 2500 references, Siu has organized the material into five parts that contain 12 chapters: Part A: Introduction (I. Cellulose Decomposition in Nature); Part B: Structure and Properties of Cotton Fabrics (II. Morphological, Physical and Chemical Features of Cotton Fibers, III. Structural Features of Cotton Fabrics); Part C: Causal Organisms (IV. Morphological and Taxonomic Aspects, V. Effect of Nutritional and Environmental Factors on Cellulolytic Activity of Microorganisms); Part D: Mechanism of Degradation (VI. Gross Attack on Fabrics, VII. Biochemical Transformation of Cellulose Molecules); Part E: Methods of Prevention (VIII. Theoretical Considerations, IX. Methods for Testing Microbiological Resistance of Cotton Textiles, X. Prevention of Microbiological Damage by Physical Barriers, XI. Prevention of Microbiological Damage by Toxic Inhibitors, XII. Prevention of Microbiological Damage by Chemical Modification).

In the reviewer's opinion, integration and interpretation of material are achieved to a considerable degree. Less satisfying is the extent to which the monograph form is burdened with an overlong exposition of background material. Many pages are given to generalized discussions of physical and chemical characteristics and requirements of microorganisms when summary statements of fact could have provided sufficient foundation without lessening the emphasis on cellulose breakdown. Similarly, other elements receive what some may feel to be undue elaboration. Such qualities result apparently from the author's attempt to write for a very diverse group and do not detract seriously, however, from the sound merit of the book. Indeed, most of the material has been organized with excellent clarity, and it is to be regretted that the price of the volume will limit its distribution.

WILLIAM D. ROSENFELD

California Research Corporation
La Habra, California

● ÉLÉMENTS DE SCIENCE ET DE TECHNOLOGIE DU CAOUTCHOUC

Issued by L'Institut Français du Caoutchouc under the direction of *Jean Le Bras,* Inspecteur Général Scientifique Société D'Éditions Techniques Coloniales, in collaboration with twelve French rubber chemists and technologists. Société D'Éditions Techniques Coloniales, Paris, 1951. xviii + 336 pp. 183 figs. 43 tables. 15.5 × 25 cm.

This book, written under the direction of one of France's most outstanding rubber scientists and technologists, is actually a compilation of lectures given in the elementary course on the science and technology of rubber offered by the Institut Français du Caoutchouc. In 15 chapters it discusses the historical development of the rubber industry and the basic discoveries, like mastication and vulcanization; the sources and production of crude natural rubber and rubber latex; latex and its properties; crude rubber and its physical properties; the composition and structure of rubber; its chemical properties; vulcanization; compounding ingredients and their main purpose; equipment used in the production of rubber goods; analysis and standard tests for rubber and rubber latex; hard rubber, reclaim, and chemical rubber derivatives; the direct application of rubber; synthetic rubbers; theoretical aspects of the chemistry of rubber; and the use and applications of rubber in general.

The authors certainly deserve credit for having written such an excellent survey of the course offered to their students. They have put at the disposal of those who can read French a concise and up-to-date treatise on the science and technology of rubber. It also deserves attention as a reading assignment in French at our technical institutes. American rubber scientists and technologists will wish that the book included references to original publications. The appended bibliography is limited to books published in French.

ERNST A. HAUSER

Massachusetts Institute of Technology
and Worcester Polytechnic Institute
Massachusetts

● MATERIALS TECHNOLOGY FOR ELECTRON TUBES

Walter H. Kohl, **Consultant to the Director of Research, Collins Radio Co., Cedar Rapids, Iowa. Reinhold Publishing Corp., New York, 1951. xv + 493 pp. Illustrated. 16 × 23.5 cm. $10.**

This book, describing the properties and applications of solids for electron tubes, is the first American book of its kind available to the general public. Although this volume presents much new material, its main value lies in the compilation of data for ready reference. Manufacturers of electron tubes have prepared their own engineering manuals in the past but none of them approach the compactness, conciseness, and availability of this book.

It is no exaggeration to expect that this text will become the "Bible" of tube engineers concerned with the properties of pertinent materials. It will be the starting point of the quest for information. The answers will, in the majority of cases, be found here or in the cited references. While the material was prepared by a man with extensive experience in the electron tube field, the scope of the contents extends beyond this field. Experimenters, physicists, chemists, technicians, and teachers in other branches of science can spend many profitable hours with this text.

There are 19 chapters which discuss seven general topics: glass, structure of solids, metals, ceramics, phase rule, vacuum technique, and thermionic emission. The first six chapters describe the physics of glass, annealing, strain analysis, glass-to-metal seals, electrical conduction, and glass in radiation fields, respectively. These topics are well chosen and are of great importance in the tube industry. However, it is this reviewer's opinion that the book would have been even more valuable had it included some omitted materials such as gases, phosphors, mercury, caesium, etc., instead of devoting about 31 per cent of the text to glass. The references list some excellent volumes readily available on the subject of glass whereas there is a scarcity of classified information on the other subjects.

Chapter 7 presents a review of metallurgical concepts of structures. Methods of analysis such as sections, X-ray and electron diffraction, spectrographic, fractography, crystallography, petrography are listed, in addition to chemical analysis. Space lattices and crystal systems are tabulated. A classification of crystals according to the prevailing bond is given. The band theory of solids is of necessity presented in a brief but meaty treatment.

A chapter is devoted to each of the following elements: tungsten, molybdenum, tantalum, nickel, copper, and carbon. Physical, chemical, and electrical properties are given in a manner that is concise but still rather complete. Reactions of oxides and compounds will be of particular interest to chemists. Gas evolution and references to the works of Norton, Marshall, Gulbransen, and others will also be of interest. Each of these materials has one or more characteristics that have acquired particular significance in the tube business. This text brings out these important qualities, like the crystal structure of tungsten, oxygen content of copper, controlled impurities in nickel, etc. The information is presented in perspective not found in other books.

Since metals are so important in the vacuum tube industry, the author has wisely devoted over one-third of his book to them. The importance of joining metals by soldering cannot be overemphasized. The material on this subject has been judiciously chosen and clearly presented. Theoretical discussion has been aided by many references to the phase diagrams so indispensable to chemists and metallurgists. A table of solders and brazing materials is given which lists 128 compositions with melting points ranging from 38° to 1966°C. It is regrettable, but not the fault of the author, of course, that there are so few solders in the 400–600°C. and 1500–2300°C. ranges.

Ceramics, mica, and ceramic-metal seals are discussed in two very valuable chapters. The author has recognized that the use and importance of ceramics has increased rapidly in recent years: "The use of ceramics for tube envelopes has gained in importance over glass since the later is too fragile and has a low softening point. If we add to this enumeration the extensive use of ceramics for circuit components, such as resistors, condensers, transducers, printed circuits, coil forms, etc., there can be little doubt that a ceramist should be included on the staff of a modern tube laboratory." Raw materials, fabrication methods, special bodies, applications, and properties are all treated. History and development of ceramic seals is summarized up to the time of publication.

Although Gibbs' Phase Rule is well known to chemists Chapter 17 presents a valuable review for them; for one less experienced in this field it provides a good introduction. The phase rule is useful in the understanding of glass composition, alloy phases, and the equilibria formed in brazing operations. Data in the chapter on high-vacuum technique pertain to fore pumps, diffusion pumps, greases, waxes, cements, and getters. Most of this information is presented in tabular form, which conserves space while at the same time presenting extensive data. A beginner may need the aid of more descriptive texts before obtaining full value from this chapter, however.

High lights and recent developments in electron emission are reported in the last chapter. Here again previous experience is necessary for full appreciation of the contents. Allusion is made to new materials and developments which are to be further explored in the future. These developments include thoria, carbides, borides, cermets, "L" cathodes, and new facts concerning the oxide cathode.

In summary, this book fills a long felt need in the electron-tube industry. It is a book that scientists will want to own individually and keep handy for ready reference. Because its stated aim is the scientific treatment of the main solids of electron-tube construction other allied subjects have not been thoroughly covered. A valuable companion volume might describe processes, techniques gases, and secondary solids.

LEO J. CRONIN

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● DIE CHEMISCHE AFFINITÄT

Egon Wiberg, **Professor of Inorganic Chemistry, University of Munich (Germany). Walter De Bruyter & Co., Berlin, 1951. xii + 254 pp. 56 figs. 281 equations and tables. 14 × 22 cm. DM 24.**

THIS book is based on a course which the author offered as an experiment during the summer of 1950 at the University of Munich. He tried to explain the problems of chemical affinity and the questions connected therewith to his students by limiting his discussion to the most basic facts and by replacing the abstract with factual evidence. To accomplish this he treated the prevailing conception of entropy, for example, as the capacity factor of thermal energy analogous to the concept of the amount of electricity in electrostatics or electrodynamics, or the concept of water volume in hydrostatics or hydrodynamics.

Professor Wiberg derserves credit for having had the courage to disregard the conservatism which is so typical of higher education. With his book he has offered factual proof that far more can be accomplished by broadening the scope of our education, instead of making living slide rules or living textbooks out of our younger generation. This book deserves the special attention of every educator who is teaching physical chemistry in the United States.

ERNST A. HAUSER

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
AND WORCESTER POLYTECHNIC INSTITUTE
MASSACHUSETTS

● THE CHEMICAL TECHNOLOGY OF DYEING AND PRINTING. VOLUME II

Louis Diserens. **Translated and revised from the second German edition by *Paul Wengraf* and *Herman P. Baumann.* Reinhold Publishing Corp., New York, 1951. xii + 446 pp. 16 tables. 16 × 23.5 cm. $12.**

THIS text is an extension of the first book, now considered as Volume I, which treated vat, sulfur, indigosol, azo, and chrome dyestuffs. The present book comprises a translation and revision of the remaining important chapters of Diserens' "Die neuesten Fortschritte in der Anwendung der Farbstoffe" with newer references added to bring the material up to date.

In eight chapters, 6 to 13, the history, theory, technology, and formulas for dyeing and printing substantive and basic dyestuffs, aniline black, and acid dyes are presented; also the use of various types of dyestuffs in the dyeing and printing of cellulose acetate,

and the newer synthetic fibers, and finally the printing of pigment colors and special printing effects are considered. Methods of dyeing and printing such fibers as the superpolyamide, vinyl polymer, terylene, alginate, silicone, and glass fibers are included. The chapter on the printing of pigments and metal powders is of considerable interest as is also the material on plissé, parchmentizing, offset printing, the orbis method, and gauffrage effects. The value of the text is increased by the inclusion of tables of auxiliaries, giving manufacturer's names, the constitution of the product, its application and literature references, following the chapters on direct, basic and acid dyestuffs, aniline black, and acetate rayon. Voluminous references to the literature and to patents are given. The tendency to refer to foreign patents rather than U. S. patents is evident, and many formulas contain foreign trade name products. Nevertheless, Volume II brings together a wealth of material which is well presented from the technological point of view.

The translators are to be commended for making available in the English language such a comprehensive and up-to-date treatment of the subject matter.

W. W. RUSSELL

Brown University
Providence, Rhode Island

● PHASE MICROSCOPY

***A. H. Bennett, Helen Jupnik, H. Osterberg,* and *O. W. Richards,* American Optical Co., Stamford, Conn. J. Wiley & Sons, Inc., New York, 1951. xiii + 320 pp. Illustrated. 15.5 × 23.5 cm. $7.50.**

The phase microscope was developed commercially during the period when the electron microscope was receiving the lion's share of attention. Many of the publications in the field of phase microscopy have not been readily accessible to the average worker or student. "Phase Microscopy," the first book of its kind, is a very timely compilation of the underlying principles, construction and applications of this much neglected tool. For those who have been working in this field, with complacent satisfaction, it rekindles the desire and need for further exploitation of the instrument.

The team of authors has produced expert coverage of the field. The various chapters are individually written by the authors. Whatever nonuniformity and repetition have been introduced as a result of this method of presentation are perfectly palatable. While a detailed mathematical treatment of the diffraction theory of image formation in the phase microscope is given in the appendix, fortunately a simplified theoretical understanding of the principles involved is given in the first few chapters.

Of particular interest to the microscopist is the description of the alignment and adjustment of the phase microscope. The chapter on techniques of phase microscopy contains material not heretofore collected, much of which is applicable to regular light microscopy.

The discussion of the salient features of phase microscope equipment supplied by different manufacturers today is particularly timely now that practically all of the leading microscope manufacturers have introduced instruments in this field.

CHARLES MARESH

American Cyanamid Company
Bound Brook, New Jersey

● MÉTALLOGRAPHIE: LES DIAGRAMMES D'ÉQUILIBRE PAR QUESTIONS ET RÉPONSES

***A. Glazunov,* formerly rector and professor at the School of Mines of Pribram. Preface by *G. Chaudron,* professor at the Sorbonne and president of the French Metallurgical Society. Translated into French from the original Czech by the author. Dunod, Paris, 1951. vii + 240 pp. 425 figs. 15.5 × 24 cm. Paper bound. 1,280 francs.**

The author of this interesting book presents the whole theory of equilibrium phase diagrams of binary systems, metallic alloys particularly, as well as numerous exercises, through the device of questions and answers. The result is a pedagogical feat. Starting with elementary matters of definitions, the phase rule, and other fundamentals, the subject matter rises from one level of complexity to a higher one in imperceptible steps and culminates in the detailed study of diagrams such as that of copper and tin and that of gold and zinc. Many other important diagrams are discussed throughout the book. The first part, entitled "Introduction to Metallography," consists of 104 questions and answers with 7 additional ones on calculations of composition. Questions 75 to 85 on thermodynamic matters are weak and their answers oversimplified to the point of being inaccurate. They should have been left out. The second part entitled "Metallography—Description of Diagrams" consists of 333 questions and answers distributed between the following topics: solidification of solutions; solidification with eutectic; constituents with two or several allotropic forms; formation of chemical compounds; chemical compounds decomposing before melting; formation of total solid solutions; complete or partial immiscibility in the liquid state; decomposition of two solid solutions; variation of physical properties in terms of composition and structure; interpretation of complex diagrams with daltonide and berthollide phases.

The book contains no references to other books nor to the original literature. It is to be regarded as a strictly didactic contribution. Professor Chaudron, in his enthusiastic preface to this French translation from the original Czech, expresses the conviction that the book will greatly improve the understanding of equilibrium diagrams among French engineers and metallurgists. It might well play the same role in other countries.

PIERRE VAN RYSSELBERGHE

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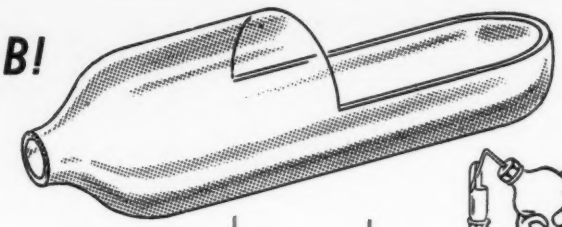

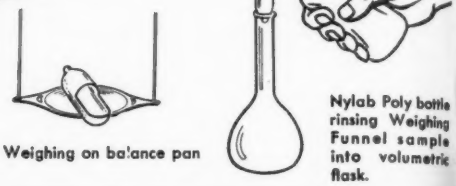

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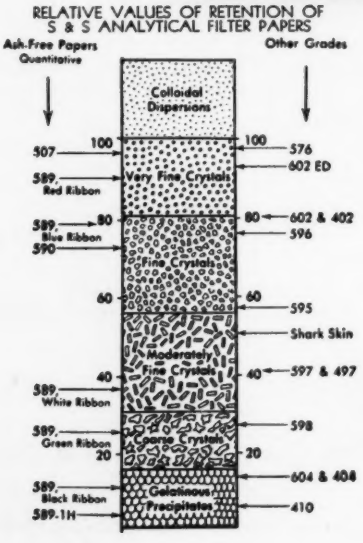

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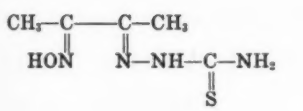

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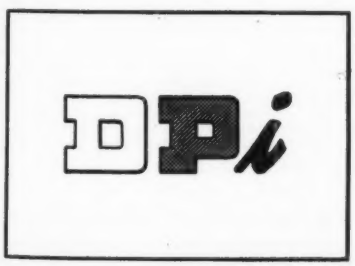

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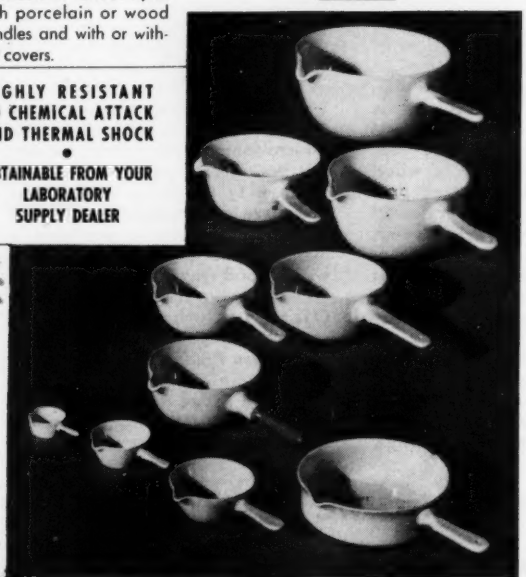

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Out of the Editor's Basket

New Stirring Device

A new and most ingenious stirring device, the Rotostir, appears to offer several worth-while advantages. The Rotostir is quite simple in design yet is said to provide a wide variety of agitation with improved distribution of stirring, particularly around the bottom periphery of the flask where heat is usually applied.

The device itself consists of an aluminum disc with a $^1/_4$-inch glass rod and rubber stopper. The stopper is made with an internal membrane which provides flexible movement of the glass rod as well as effective sealing. One each of sizes 6, 7, 8, and 9 stoppers are supplied to fit various size flasks.

The straight-glass stirring rod rotates in such a manner as to simulate hand stirring. Almost the entire length of the rod moves and it may be adjusted while in operation so as to provide more or less sweep as desired. The glass rod is a part of the flask and stopper. It is easily removed from the stirring motor when necessary and it is easy to assemble and clean.

According to the manufacturer, the Rotostir is excellent for stirring in chemical reactions which require reflexing. It is also recommended for reactions that require distillation under reduced pressure since the rod is sealed by the rubber membrane of the stopper.

Further details about the Rotostir may be obtained by writing Central Scientific Company, 1700 Irving Park Road, Chicago 13, Illinois.

Corrosive Sulfuric Acid

Following the usual lecture demonstration of the effects of concentrated sulfuric acid upon wood, paper, and cane sugar, the lecturer can show that its corrosive action is initially slow. To do this, place 1 or 2 ml. of the acid (density 1.84) in the dry, hollowed palm of the hand; dip the head of a chlorate-type safety match into it showing that it is sufficiently reactive to cause ignition; and then wash the hand with a copious stream of tap water. No sensation or injury occurs if the experiment is conducted in less than 15 seconds.

This demonstration was suggested by Dr. A. Viswanathan, Madras Christian College, Tambaram, South India.

New Laboratory Thermometers

The use of a new permanent filler for lines and numbers on laboratory thermometers has been developed by Kimble Glass, division of Owens-Illinois Glass Co.

It is claimed that the new development adds to the life of the laboratory thermometer and overcomes a common fault—the disappearance of markings due to the erosion of acids and organic elements.

The new-type thermometers were tested under conditions more extreme than those encountered in normal use. To determine permanency of the fillers they were immersed in acids, alkalies and organic solvents at elevated temperatures for long periods of time with no damaging effect on the filler. Some fading resulted from boiling alkali which characteristically attacks glass, but the filler proved as permanent as the thermometer itself.

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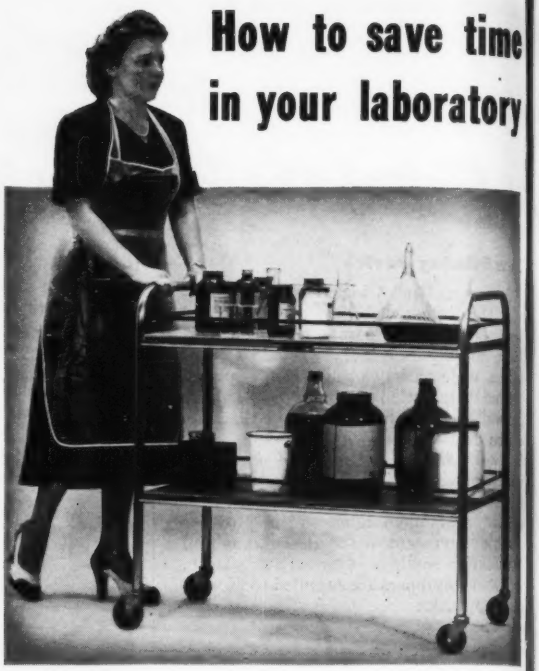

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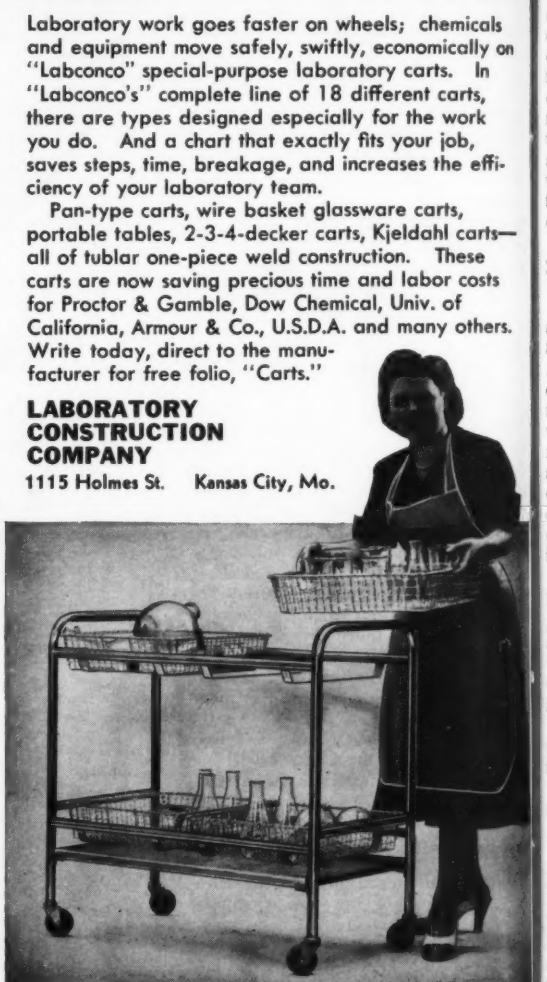

Photoelectric Colorimeter and Turbidimeter

Hellige, Inc., Long Island City 1, N. Y. has introduced the new "Chromatron" Photoelectric Colorimeter and Turbidimeter. The instrument features selective color filters covering the entire visible spectrum, square and round absorption tubes, hermetically sealed photocell, extra large meter, and automatically focused bulb. According to the manufacturer,

simple 3-step operation gives instantaneous, accurate readings. The instrument operates on all standard voltages and frequencies, as well as from two small dry cell batteries for complete portability. It is said to be extremely sensitive to meet the highest requirements of laboratory accuracy, yet exceptionally rugged to withstand rough handling in the field or factory. The colorimeter is described in catalogue No. 925.

New Literature

Volume 4, Number 1, of *Lanco Apparatus News* has just been published by Arthur S. LaPine & Company, 6001 South Knox Avenue, Chicago 29, Illinois. It is an eight-page bulletin describing many new items of equipment. The new Kinney vacuum pump, electric stopwatches, aluminum speed desiccators, new filamatic automatic pipetting machine, polyethylene bottles and funnels, bacteriological incubators, air velocity meters, and many other items are illustrated and described in detail.

The latest issue of *Witcombings*, the house organ of the Witco Chemical Company, is now available. It contains articles on Chicago, "Giant of the Plains"; "A Brighter Future" for industry through the use of color; the origin of the plastics industry; the newest products from the Witco Labs; and significant items combed from news of the chemical and allied fields.

Copies may be obtained by writing to Witco Chemical Company, 295 Madison Avenue, New York 17, New York.

A complete catalogue of Whatman Filter Papers has just been published by H. Reeve Angel & Co., Inc., 52 Duane Street, New York 7, N. Y. The catalogue describes the Whatman products, and makes suggestions regarding grades to be used for various determinations and analyses. Copies may be had upon request to H. Reeve Angel & Co., Inc.

Eastern Chemical Corporation, 34 Spring Street, Newark 2, N. J., recently published their new catalogue on organic laboratory chemicals. It contains a list of about 1000 chemicals now available from this company. Copies of the catalogue may be had by writing to the above address.

Dynamic Atom Model

The Underwood Atom Model consists of an assembly of balls representing neutrons, protons, and electrons, the latter supported by spokes in orbits outside the "nucleus." The model is supported by a 5-ft. stand, which is collapsible. When a crank at the base is turned, the electrons move in circular orbits, tracing visible paths about the nucleus, the particles of which seem to mill about the center. The head now appears as an enlarged ball representing a dynamic Bohr atom.

In a stationary position, the model may be used to demonstrate fundamental atomic particles, electron theory of matter, release of nuclear energy, quantum theory, magnetic effect, radiation of energy, etc. The formation of a molecule may be illustrated by joining two hydrogen atom models of the oxygen model.

The model may be ordered from The University Apparatus Co., Berkeley, California, in the West, and from W. M. Welch Scientific Co., Chicago 10, Illinois, east of the Rocky Mountains.

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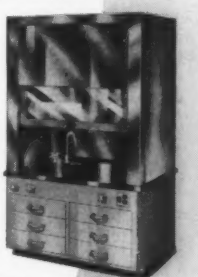

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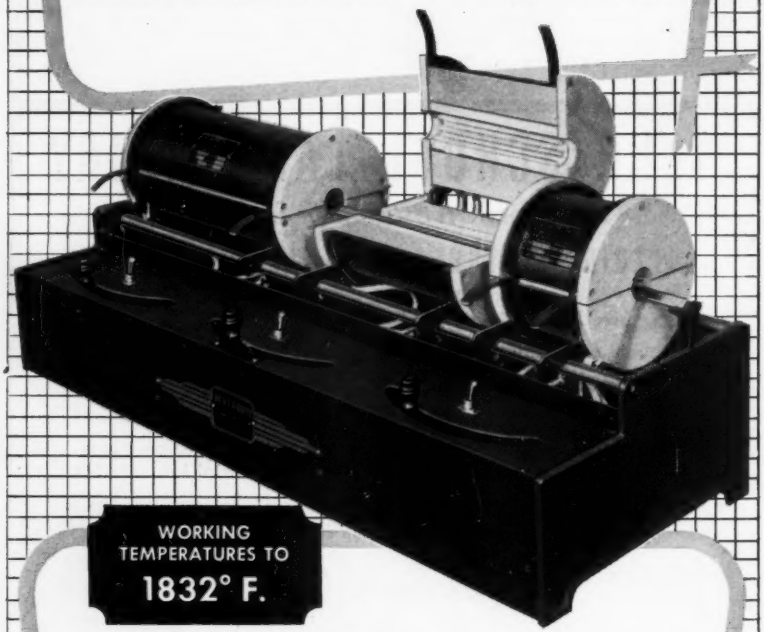

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"Any chemist at all interested in studying Russian should be much encouraged on reading Dr. Perry's chapter entitled Suggestions for Study Methods. Here the approach to study of Russian is analyzed logically and clearly. And he will certainly want to take advantage of all the useful material packed so solidly into the thoughtfully organized chapters on Vocabulary Problems, Inorganic Chemical Nomenclature, Organic Chemical Nomenclature, and Russian Grammar, as well as the Glossary of Technical Terms which contains almost 2,000 words. In fact, this book would be a good investment even to those chemists who want to learn only enough Russian to read the titles of technical articles."

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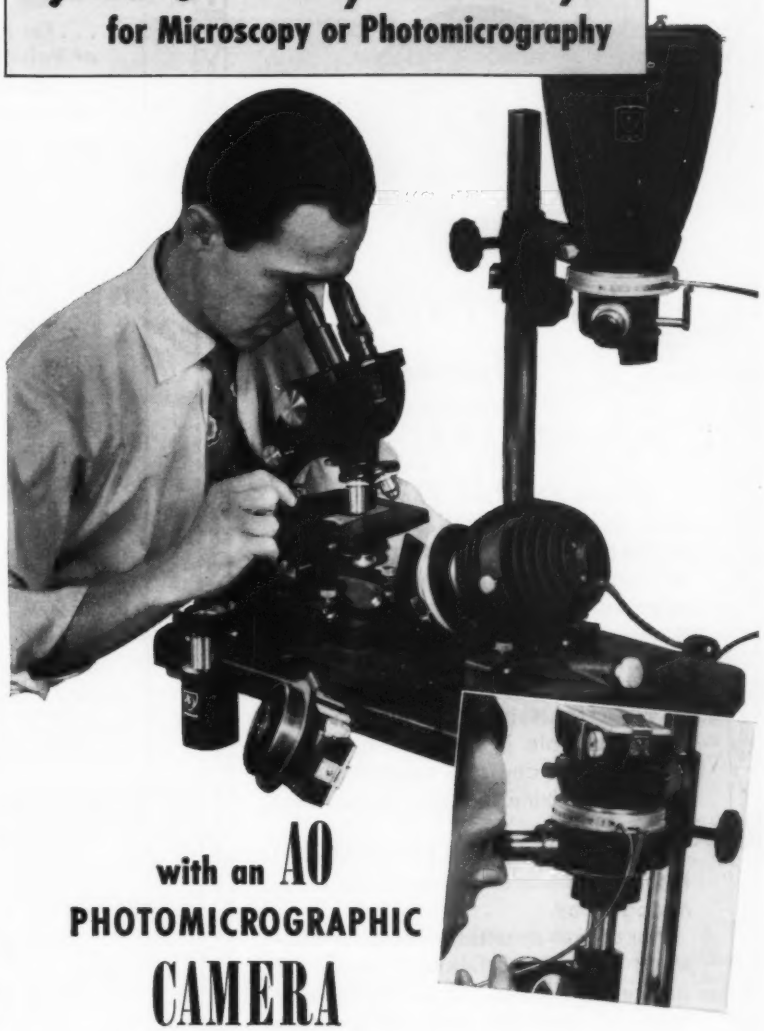

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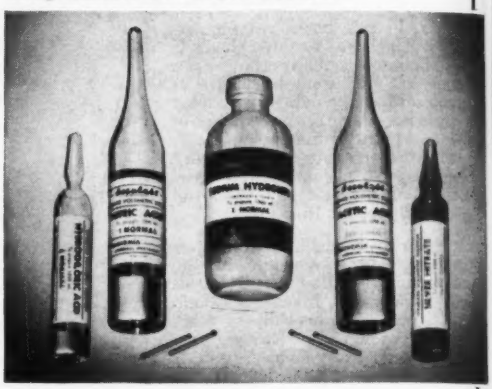

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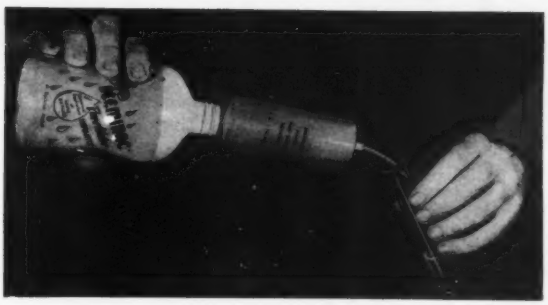

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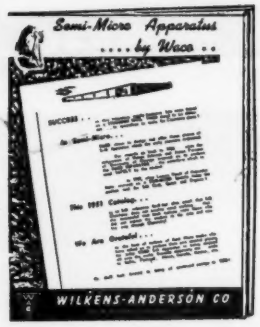

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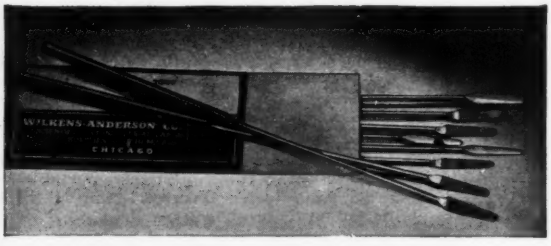

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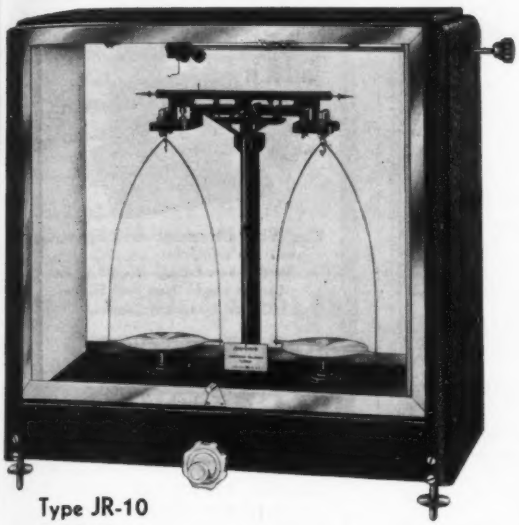

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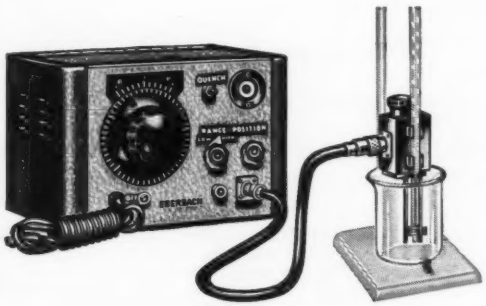

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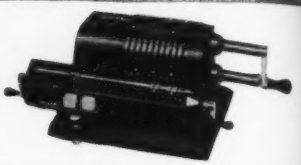

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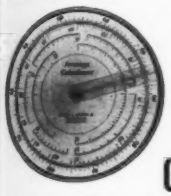

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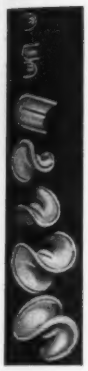

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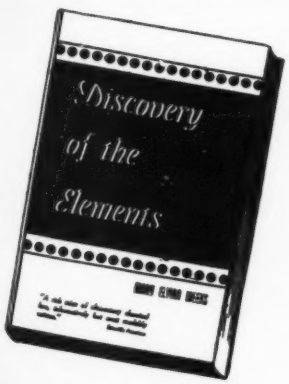

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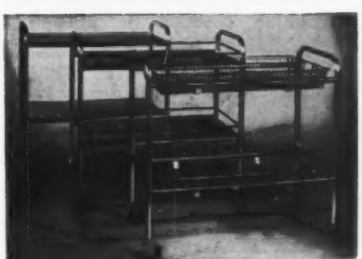

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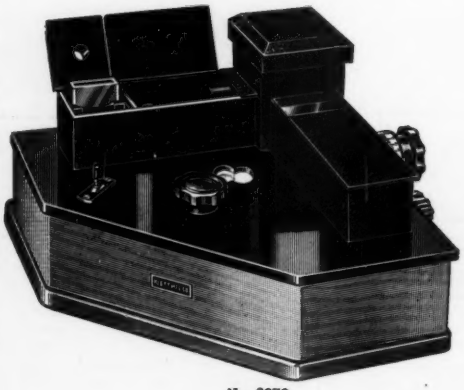

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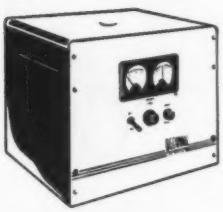

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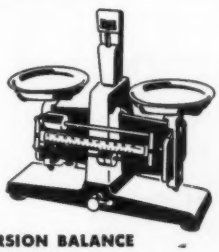

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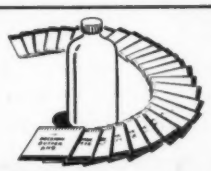

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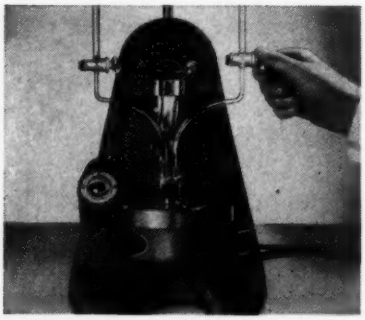

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